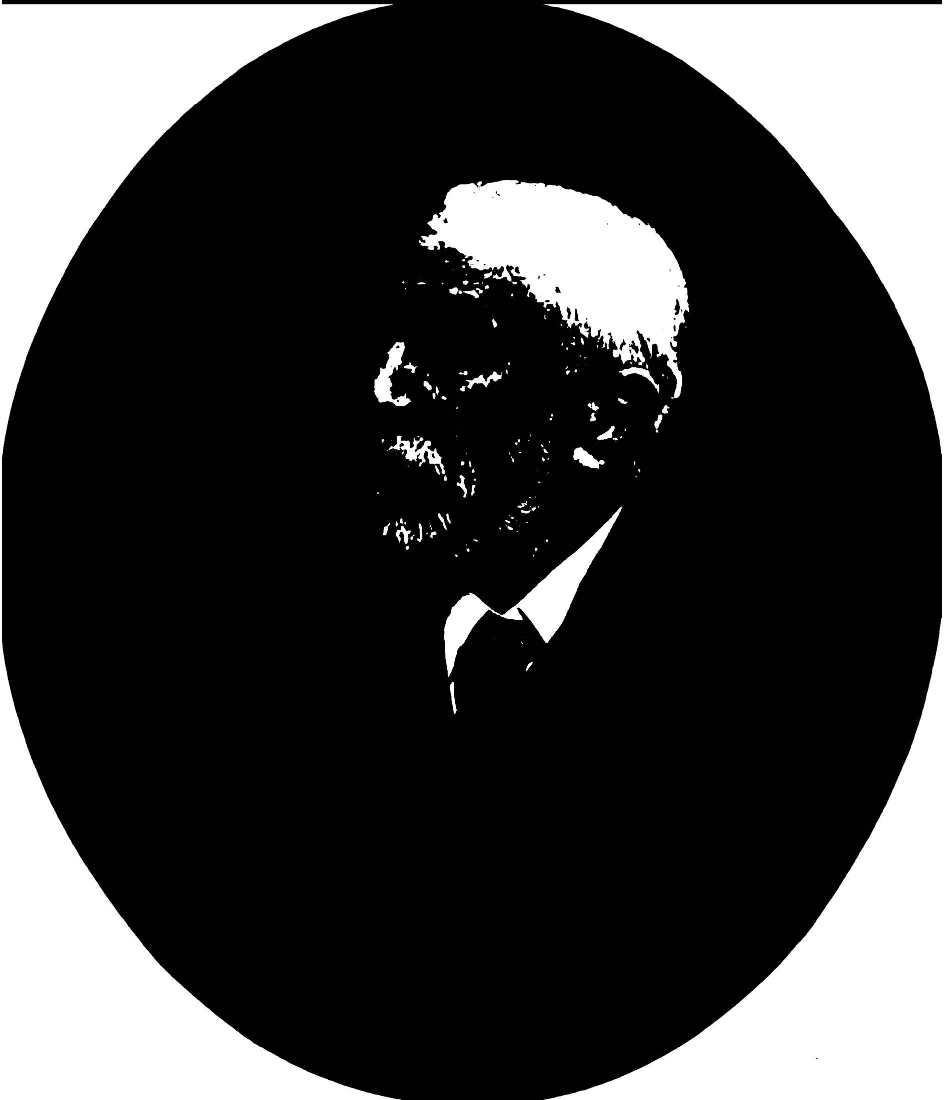

Minutes of proceedings of the Institution of Civil Engineers

Institution of Civil Engineers (Great Britain)

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SIR WILLIAM MATTHEWS. K.C.M.G.

ELECTED PRESIDENT 1907.

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MINUTES OF PROCEEDINGS

OF

THE INSTITUTION

OF

CIVIL ENGINEERS;

WITH OTHER

SELECTED AND ABSTRACTED PAPERS.

VOL. CLXXI.

EDITED BY

J. H. T. TUDSBERY, D.Sc., M. INST. C.E., SECRETARY.

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THE

INSTITUTION

OF

CIVIL ENGINEERS.

SESSION 1907-1908.—PART I.

SECT. I.—MINUTES OF PROCEEDINGS.

5 November, 1907.

Sir ALEXANDER B. W. KENNEDY, LL.D., D.Eng., F.R.S., in the Chair.

SIR ALEXANDER KENNEDY said: Since we parted last Session The Institution has sustained a loss which we may call truly, and in no merely conventional sense, irreparable, in the death of our old colleague and friend Sir Benjamin Baker. It is unnecessary for me here and now to say anything of Baker's achievements as an Engineer—they are known to all of us, and as well known to the youngest of our students as to those around this table, and they will still be familiar to our grandchildren;—I would rather try to express in a few words something of the feeling with which we members of The Institution regarded him from our personal point of view.

It is 12 years since his Presidency of The Institution ended, but his influence in the Council, and through it on the general policy of The Institution, has never lessened: fortunately we were able to secure him every year as a Member of Council. At the Council table, as elsewhere, he was a man of few words—but the few words were not said hastily or without consideration. Naturally they carried the greater weight, and one can recall many occasions when his decided and common-sense opinions, tersely expressed, sufficed to decide a point about which otherwise there might have been endless verbal discussion. Of such mere verbal discussion about unimportant details he was extremely impatient, but I never knew him to lose patience in a discussion conducted seriously and to the point, even when its trend was not in accordance with his own opinion. All who have ever sat near him at this table will remember his dislike to waste of time in the conventional complimentary remarks and generalizations with which so many speakers find it necessary to begin a contribution to any discussion in this room.

He was a Member of Council for 25 years, and his membership throughout was no merely formal affair. He took as much interest in the affairs of The Institution as in his own professional work, and more than that could hardly be said. He had an excellent memory also, and was an unfailing source of information as to precedents, and in this way his experience was a great help to the preservation of that continuity of policy which is so important in an Institution like this. But he was singularly free from any feeling that a thing was right because it had been done before, even if it had been done often: the changes in the constitution of The Institution, introduced during his Presidency and largely on his initiative, remain as very conclusive evidence of his breadth of view. In the consideration of the claims of candidates for Membership also, which forms so large a part of the business of the Council, while no one was keener to detect inflated or undue pretence, no one, on the other hand, was more ready to recognize honest work, even if it were not of the highest order, and no one took a more kindly view, generally, of the claims of anyone who appeared to be a real Engineer and not a talker.

The world at large has lost one of the greatest of its Engineers; the members, and especially the Members of Council and the many engineers who worked with him in business, have lost a colleague and a helper of unrivalled experience, and quite apart from engineering, many of us—a great many, I should like to think—have lost even more—have lost a friend whose kindly talk, whose happy humour, whose ready advice can never be forgotten and never be replaced.

On the 12th June last the Council passed the following resolution, which was sent to Sir Benjamin's representatives: "That the Council on its own behalf and on that of The Institution record their deep regret at the death of their eminent Past-President and colleague, Sir Benjamin Baker, K.C.B., whose intimate and continuous association with the work of the Council for a period of 25 years has been of the utmost value to this Institution, as his engineering achievements have been of utility and benefit to the world."

Sir Alexander Kennedy remarked that it afforded him great pleasure to rise for the last time as President of The Institution, because the object of his rising was to introduce his successor, his friend, the friend of all the members, one of their most distinguished engineers, Sir William Matthews. It was quite unnecessary that he should say anything about Sir William, but he hoped Sir William would say something about himself.

Sir WILLIAM MATTHEWS, K.C.M.G., President, then took the Chair.

Sir John Wolfe Barry, K.C.B., Past-President, thought Sir Alexander Kennedy was the only person in the room who felt any pleasure in contemplating the fact that he was leaving the Chair. He was sure the members were all exceedingly sorry to lose him, and the only comfort they had was that he was to be replaced by another valued friend, whom they all admired in his present capacity of President as they had admired him in all the steps which he had gradually taken in The Institution. His object in rising was to ask the members to accord their thanks to Sir Alexander Kennedy for the way in which he had served The Institution by occupying the Chair for the past 12 months, and he did not think he should have any difficulty in carrying the whole of the meeting with him when he said that they felt deeply grateful to Sir Alexander Kennedy for all the hard work he had gone through, and for the ability, the urbanity, and the courtesy with which he had treated every member of The Institution, not merely at home, but in every part of the world in which the Institution's influence was felt. He knew from experience that the work of the Presidency of The Institution was no light task to throw upon a man, and particularly on one who had so many urgent calls upon his time as Sir Alexander Kennedy had, and he thought the members would only be doing what they all felt to be right and a pleasure if they accorded to Sir Alexander a very cordial vote of thanks for what he had done for The Institution. He therefore moved: "That the members present at this meeting desire on behalf of themselves and others to record their high appreciation of the services rendered to The Institution by Sir Alexander B. W. Kennedy during his term of office as President."

Mr. Charles Hawksley, Past-President, observed that it was a great satisfaction to him to have been allowed the privilege of seconding the resolution which had been so ably proposed by Sir John Wolfe Barry. He did not know that he could add much to what Sir John had said, but he would like to express his own feelings of friendship towards the retiring President, and to say how much, in common with other members, he had admired him, did still admire him, and hoped long to have the opportunity of continuing to do so. He had conducted the affairs of The Institution so ably that he deserved the thanks and good will of every member. He had been not only a very able President but also a good friend to all.

The resolution was carried by acclamation.

Sir Alexander Kennedy thanked the members very much for the kind way in which they had spoken of his year of Presidency. He had heard it said that it required more courage for a soldier to run away than to fight, and he thought it was some such feeling, rather than any feeling that he was a fit man to be President, that had made him accept the Presidency when it came to be offered to him. No man who had not passed the Chair could know what a heavy responsibility it involved. He said nothing about the amount of work, because that was expected; but that serious feeling of responsibility had made the past year to him the most engrossing and interesting year that he had ever lived, and certainly the year in which he had felt more responsibility than at any other time—not excepting even certain early years at University College when his sole ambition had been to keep a few weeks ahead of his students, so that those beautiful souls athirst for knowledge should not find him out! They did not find him out until later, so that it was all right. In saying that it was a pleasure to him to come to the end of his term of office, he was sure the members would understand that he really meant that it was a relief to finish a year of considerable strain and stress, for a man who was no longer quite young, and not very strong at that. In other ways the year had been a year of great pleasure to him, and for that pleasure he had to thank many: he had to thank in the first instance his colleagues around the Council table for their helpfulness and kindness, their forbearance and co-operation; and he had to thank, as everyone of them would know, their good friend, Dr. Tudsbery, for many things: his great talent for organization, which had been seen particularly that year in the Conference and in the Conversazione, but which the President saw continually, had made simple and smooth all sorts of things that otherwise might have been difficult if not almost impossible. After the Secretary, he had to thank the whole of his staff, from the highest to the lowest, for kind and prompt helpfulness on every occasion when anything was wanted. Finally, he had to thank the members of The Institution, who had managed to keep the year entirely free from anything disagreeable, which really showed how very good they had been to their President. He had also to thank the members very much for having put up with him and with his long speeches for a year. Although it had been a year of great pleasure to him, he was very glad that it was at an end, and he congratulated his friend Sir William Matthews on now beginning a similar career.

The PRESIDENT then delivered the following Address to the Members:—

My predecessor in this Chair has just referred in sympathetic terms to the great loss which The Institution and the profession generally have sustained in consequence of the death of our distinguished Past-President, Sir Benjamin Baker. Having worked with Sir Benjamin and sought his advice in times of stress, I may be permitted to record my sense of the great value of his co-operation and the wide extent of his knowledge and experience. His was a warm and true heart, leading to acts of kindness and sympathy, whilst his achievements have thrown lustre on the profession, and have rivalled the most notable enterprises of its founders. Although the Forth Bridge and the Assuan Dam are monuments in themselves to his skill and courage as an engineer, the movement which is on foot to perpetuate his memory by the provision of a suitable memorial in Westminster, will be gratifying to his many friends and to the profession at large.

Another subject to which I desire to refer with regret, is the ill-health of our esteemed Vice-President, Mr. Galbraith, which has prevented him from occupying the position which I hold to-night. Mr. Galbraith has been actively engaged during a long career in the construction of extensive and useful public works, and it is alike regrettable to us, as it must be disappointing to him, that towards the close of his busy life, under medical advice, he is, for the time, unable to accept the highest position which can be accorded to any engineer, namely, the Presidency of this important Institution.

The kindness and indulgence of my professional brethren having placed me next on the list of Vice-Presidents to my friend Mr. Galbraith, on his being unable to occupy the Chair for the present year, I find myself in the position we should have wished him to fill. Having been connected with The Institution for 38 years, first as an Associate, then as Member, afterwards as Scrutineer, Auditor, Member of Council and Vice-President, I nevertheless accepted with considerable hesitation the nomination of the Council to preside over The Institution during the present Session, although with the fullest sense of the great honour conferred upon me in following to the Chair so many distinguished engineers. I trust, however, that the friends who know me best will pardon my temerity in accepting this honoured position, and that all will extend their support and forbearance, which will be indeed help-

ful to me in fulfilling, as far as possible, the important duties the office.

After 80 years of active work and development The Institutic still continues to expand, extending its influence both at hon and in our imperial possessions wherever engineering work has t be done. The Institution is recognized as the parent society representing the general practice of engineering, and happil embraces members of what were formerly regarded as allied bu distinct branches, namely, mechanical, electrical, metallurgical an mining engineering, and also our scientific brethren who ar associated with naval architecture. The weaving together int one harmonious whole of the various branches of the professio in no sense weakens the activity of what may be termed th sectional societies, which still flourish, as they deserve, in pro moting their special objects.

Another healthy development in connection with The Institutio is the assemblage of engineers at periodical Conferences in West minster, the first of which was held in 1897 under the Presidency of Sir John Wolfe Barry; the recent gathering, presided over by Sir Alexander Kennedy, has been attended by marked success These assemblages of engineers, and the professional intercourse and interchange of thought arising therefrom, cannot fail to be of benefit, bringing up to date, as they do, the knowledge and experience of the subjects discussed in the respective sections.

Not a few of the most important works in the British Colonies have either been designed by, or submitted for the opinion of, leading engineers at home. Nevertheless, especially in recent years and in growing numbers, works of considerable magnitude and importance have been carried out, which owe their inception and execution to the talent of our colonial brethren, and entitle them to our cordial congratulations. We can readily understand how valued by such men are the records of works to be found in our Proceedings, and it is to be hoped that, in recognition thereof, they will continue to contribute to The Institution, Papers on their own special engineering achievements.

Sir Benjamin Baker, in his Address in November, 1895, alluded to the problems of vast interest still awaiting solution in all branches of physical science and engineering, and pointed out that the supply of labourers in these fields is practically unlimited. He considered that the number of engineers, both at home and abroad, would doubtless increase in a more rapid ratio than the work to be performed, and that consequently the competition would be more severe

in the future than in the past. This forecast has been borne out by experience. Since 1895 the class of Members has increased 22 per cent., Associate Members 18 per cent., and Students 74 per cent. Notwithstanding this accession to our ranks, and the number of professional men seeking engagements, it is not always easy, when the services of a competent resident or assistant engineer are required, to come in touch readily with the right man. It may be that benefit would result, especially to young engineers, if they more fully availed themselves (without setting up an employment agency within our walls) of the means which Dr. Tudsbery adopts for bringing together those who he may know to be in want of professional assistance and those who are in a position to supply it.

An important subject which has for some time occupied the earnest consideration of the Council is the acquirement, it is feared at no distant date, of the building in which we are now met, for the extension of the new Government offices. Various sites have been considered, but naturally it is preferred that one in Great George Street should be selected, in view of the traditions of The Institution being so intimately bound up with that locality. It is therefore hoped and expected that a site in that street will be transferred by the Government to The Institution on reasonable terms, prior to the definite issuing of the mandate to relinquish our present premises.

Many previous occupants of this Chair have made the subjects of their Presidential Addresses to refer more particularly to those branches of engineering work with which they have been most closely associated. Brilliant exceptions to this procedure may be quoted in the Addresses of my two immediate predecessors—Sir Alexander Binnie and Sir Alexander Kennedy. In my case, however, I have had to fall back, mainly, on the more restricted purview of engineering which belongs to the practice of that branch of the profession with which the work of my life has been principally connected. I therefore propose in the following remarks to refer especially to "some branches of engineering work which are associated with over-sea traffic." I allude to "some branches," inasmuch as it would not be possible in the time allotted to the delivery of this Address, to do more than refer to a few sections of Engineering which bear on this subject, and only then to touch upon them in general terms.

In a memorandum written by Tredgold when application was about to be made for a Royal Charter for The Institution, he stated that

the most important object of Civil Engineers is to improve the means of production and of traffic in states, both for external and internal trade. He mentioned that the enterprising Hollanders, towards the close of the sixteenth century, first separated Engineering from Architecture under the title of "Hydraulic Architecture," their example being followed in France towards the end of the seventeenth century. The practice of Hydraulic Engineering was evidently considered in those days to be of pre-eminent importance, and with that subject, I take it, waterways and the conduct of commercial pursuits associated therewith, may be not unreasonably regarded as allied.

Sir Alexander Binnie referred in his Address to Smeaton as the father of our profession—a man of high intellect and one of the most steadfast workers of his time. Smeaton, who was born in 1724, had, as is well known, an intimate connection with lighthouse-construction, sea-works, and the provision of commercial waterways. When we examine the works of the old masters who founded the profession, we are brought face to face with their extraordinary natural resource and great courage in procedure in the absence of scientific data, and we cannot but be filled with admiration for their achievements. In this connection, I may observe that I had occasion recently to inspect a canal which was carried out in Derbyshire by Jessop more than a hundred years ago, and can appreciate the difficulties under which such works were constructed in those days, and the great skill which was required for their execution. Then, as now, the life of an Engineer was made up of a continual struggle against obstacles, calling for the exercise of sound judgment and unremitting watchfulness if successful results were to be attained.

The Development in the Dimensions and Tonnage of Steamships.

Our Past-President, Sir William White, in his Address 4 years ago, gave some particulars of British-owned steamers in 1828—the year when the Charter of The Institution was granted—and mentioned that at that time the largest steamer afloat, described as a "leviathan" and the "wonder of the world," was running between London and Leith. She was 160 feet long, of about 500 tons burden, with engines of 200 HP., and was propelled by paddles. Sir John Wolfe Barry, in 1896, when referring to a similar subject, pointed out that in 1838 the "Great Western," of 1,340 tons register, designed by Brunel, demonstrated for the first time

the possibility of the establishment of a service of steamers across the Atlantic, and ran regularly between Bristol and New York for many years, the journey occupying about 14 days. In 1845 the "Great Britain," 2,984 tons register, also designed by Brunel, which was the first large ocean-going steamer built of iron in the Mercantile Marine, and to which a screw-propeller was applied, marked a further important step in advance. She crossed the Atlantic in about 12 days, and was regularly employed as a first-class passenger-ship for many years.

Coming to ships of to-day, the principal features in their construction which affect the work of Civil Engineers engaged in making harbours and docks are connected with the rapid growth in dimensions and carrying-power which has taken place in recent years. In every class of ship this growth has occurred. For cargo-steamers, having great dead-weight capacity, economy in cost of transport is promoted by increase in dimensions; this general principle was clearly enunciated by Brunel when the "Great Eastern" was designed, and has been confirmed by all subsequent experience. For passenger-steamers of high speed, growth in dimensions has resulted from the necessity for providing machinery of greater power and for much larger coal-space. In warships of every class the same law of development in size has been at work, the governing conditions being a desire to increase armament, protection, and speed. At present the great Cunarders "Lusitania" and "Mauretania" lead the way in point of size. Their extreme length approaches 800 feet, breadth, 88 feet, draught of water, fully laden, about 34 feet, corresponding displacement, nearly 39,000 tons, gross registered tonnage about 33,000, contract-speed on the Atlantic 24½ knots.

The "Lusitania" on her first westward voyage made the passage in 5 days and 54 minutes, her average speed being 23·01 knots. Her second westward voyage occupied 4 days, 19 hours and 52 minutes. The average speed was 24 knots, and exceeded by nearly half-a-knot the highest average speed previously attained on a transatlantic voyage by the North-German-Lloyd steamship "Kaiser Wilhelm II," which average was closely approached by the "Deutschland," of the Hamburg-America Line. This achievement will doubtless be surpassed when the machinery has been running for a longer period and the crew have more experience of their ship.

There have been rumours of vessels being projected equal in size, or even larger than the Cunarders, but until something authorita-

tive is communicated regarding these it is perhaps idle to speculate on the subject. There seems, however, to be a general impression that the next advance will be in the development of the intermediate type, of which the "Adriatic" of the White Star Line is the most notable example yet on service.

Amongst warships those of the "Dreadnought" class are the largest British-built ships yet afloat. The vessels laid down in 1906–7 are 490 feet long between perpendiculars, 82 feet wide, 27 feet draught (normal), and 18,600 tons corresponding displacement. The engines develop about 23,000 HP. The "Dreadnought" herself at her "legend" draught of 26 feet 6 inches has a normal displacement of 17,900 tons, but she reaches a draught of about 31 feet when all bunkers, and oil- and water-tanks are full, which corresponds with a displacement of about 21,800 tons. The ships of the "St. Vincent" class, about to be laid down, are said to have a normal displacement of 19,300 tons. Even these dimensions are, I understand, to be exceeded in the new American battleships, for which orders have been placed recently. The particulars of these ships are:—Length, 510 feet; breadth, 85 feet; normal draught, 27 feet; corresponding displacement, 20,000 tons. They are to have engines of 25,000 HP., giving a maximum speed of 21 knots.

Hitherto expansion in ocean liners has proceeded chiefly in the direction of length, although of late years addition to beam has also been marked. Increase in length in disproportion to draught and depth is a costly change, since the principal stresses in ships' structures arise from longitudinal bending-moments. Increase in speed, no doubt, has had much to do with increase in length; both as a means of improvement in form and diminution of water-resistance, and from the necessity for accommodation for more powerful propelling-machinery and especially for the boilers. After making due allowance for these considerations, however, it appears to be true that naval architects have been compelled to adopt lengths which they would willingly have curtailed, because of the restriction in available draught of water, and in many cases of limitation in breadth, to suit existing waterways and dock-entrances. Increase in beam has been very marked in recent types, both in ocean-going passenger-steamers and in vessels of the "intermediate" class. Increase in depth, as well as enlargement of superstructures, which are not expected to contribute greatly, if at all, to the structural strength, have also been common.

"Tramp" steamers, as distinguished from the regular liners, are

chiefly engaged in the carriage of coal, grain, and general cargoes. Large numbers of these steamers range from 3,000 to 6,500 tons capacity, the latter being considered the most suitable tonnage for general purposes. There is, however, a marked tendency to increase in the average tonnage of such steamers. Some owners in recent years have built vessels of 8,000 to 10,000 tons capacity, having every appliance for the rapid loading and discharging of cargo. Such craft are somewhat large for the facilities provided at the majority of ports; in fact, the choice of ports for these vessels is undoubtedly limited.

There has been no great development in recent years in the machinery for the propulsion of "tramp" steamers. Triple-expansion engines are still the rule, and a cargo-steamer with a speed of 9 to 10 knots per hour is considered to be the most economical for general purposes, in initial outlay as well as in cost of maintenance and consumption of fuel. Considerable advances have taken place in general knowledge in the building of engines and boilers for such vessels, and in their preservation and maintenance; consequently fewer accidents occur, and boilers, crank-shafts, and similar parts, run much longer without renewal now than formerly.

Turbine Propulsion.

The most notable feature in the propulsion of recent ships is the rapid extension of the use of the steam-turbine, and particularly of that type with which the name of Mr. Parsons will always be associated. This aspect of marine engineering was fully discussed lately at the Engineering Conference, and subsequently at the Bordeaux Congress on Naval Architecture.

In the middle of the present year, there were in service sixty-one steamers fitted with Parsons turbines, and sixty-five vessels under construction to be furnished with them. The total power of these ships approached 1,400,000 HP.; of this about 42 per cent. was in merchant vessels and yachts and 58 per cent. in warships. In the new ships of the Royal Navy reciprocating engines have given place to arbines. It is universally agreed that in swift vessels of moderate size results are obtainable with turbines which cannot be reached with reciprocating engines. It also appears to be true that in vessels of larger dimensions and exceptional speed, such as the new Cunarders "Lusitania" and "Mauretania," the use of turbines is an undoubted advantage.

In warships substantial advantages have been gained by the use of turbines, an illustration of which is found in the fact that the "Dreadnought" has only eighteen boilers, whereas with reciprocating engines, and the ship going at the same speed, it is estimated that twenty-two boilers would be required. Special turbines have to be fitted for cruising at low speeds and for reversing, but with the supplemental machines included, no greater allowance of weight and space are needed than would be necessary with reciprocating engines, and the height of the engine-rooms can be considerably reduced.

In the last "James Forrest" lecture, so ably given by Dr. Elgar, it was pointed out that in ocean liners with quadruple-expansion engines and a boiler-pressure of 210 lbs. to 220 lbs. per square inch, coal-consumption had been reduced to about 1·3 lb. per indicated horse-power-hour. The lecturer further remarked that the substitution of turbines for reciprocating engines in ocean vessels depends chiefly upon whether the consumption with turbines can be brought down to this figure, and stated that there is no satisfactory evidence that this is now practicable. It is probable, he said, that the marine turbine will be ultimately so improved as to beat the best reciprocating engines, in ocean liners, in economy of consumption, but no proof is forthcoming that it can yet do so.

Hitherto the turbine system has not been applied successfully to merchant vessels of moderate speed, but Mr. Parsons has, for some years, been at work on the problem, and has expressed his conviction that by the association of turbines with reciprocating engines more economical propulsion will be possible than by any other arrangement yet devised. It has been recently announced that Messrs. Harland and Wolff are building for the Dominion line a large steamer of the intermediate type, to trade between Liverpool and Canada, which will be propelled by a combination of turbine machinery and reciprocating engines. Other firms have been working at the same problem and various plans have been proposed. That which finds most favour is to fit two sets of balanced triple- or quadruple-expansion engines with the addition of one low-pressure turbine. Each will be quite independent of the others and will have its own shaft and propeller. It is expected that this combination will possess many advantages, the principal gain being derived from the greater expansion of steam in a low-pressure turbine.

It is a matter for sincere congratulation that the practical revolution in steamship machinery brought about by the introduction of the turbine is due to the genius of an English engineer.

Oil-Fuel and Internal-Combustion Engines.

Recent progress in the employment of liquid fuel will be best understood by considering the subject under two separate sections, namely,

(a) The use of such fuel for the purpose of raising steam to be utilized in the production of power; and

(b) The direct application of the fuel, either in a liquid or gaseous form, to the creation of power, without the intermediary of steam.

As regards the first-named section, the advance made in practical application to ship propulsion, during the last decade, has been very great, and economy as low as 0·9 lb. of fuel per indicated horse-power per hour has been regularly realized in some mercantile vessels by the system of spraying the liquid for combustion by means of hot air. It is well understood that no economical combustion can be carried out except the liquid is first "pulverized" into fine spray to facilitate its combustion, and the method of so pulverizing the liquid is of the utmost importance. The steamers of the Shell Transport Company are, for the most part, using a system of pulverization by high-pressure steam-blowers, the steam itself being chemically decomposed after it has done its work, and entering into combustion in the furnace.

Upon American steamships, which have largely used oil-fuel during the past 3 years, pulverization is obtained by hot air at considerable pressure. This air is compressed by Root blowers, and the liquid after pulverization is exposed to the further action of heated air under more moderate pressure for the purpose of directly assisting combustion. This combined system of high- and low-pressure air respectively, for the purpose of pulverization and of forced-draught combustion, although somewhat complicated, has proved highly successful and economical.

Steamers of 14,000 tons displacement have regularly and successfully made voyages the whole distance from Singapore to the United Kingdom around the Cape of Good Hope, and vessels of even greater displacement have made equally successful voyages from New York to San Francisco around Cape Horn under the system of high- and low-pressure hot air. These long voyages show the practical solution of the problem of the use of liquid fuel for steam propulsion.

Most of the vessels now being built for the British Navy are constructed, as regards the double bottom and other suitable spaces, upon an oil-tight system, so that such spaces may be made available for stowing liquid fuel when the system has become more fully developed. In large warships oil-fuel is used as an auxiliary to coal. In torpedo-boats and destroyers oil-fuel alone is used. The question of available supplies of oil is one of primary importance to its extended use as fuel.

With respect to the second section, it is not to be supposed that either the Admiralty or the authorities in the mercantile marine are assuming that the use of liquid fuel is to be confined to steam evaporation. Serious experiments are being made in the application of internal-combustion engines to marine propulsion, but hitherto the problem has not been fully solved as regards the development of large power.

The internal-combustion engine in use in motor-boats has now become familiar, and such vessels are capable not only of extremely high speed, but also of sustained endurance.

The difficulty of producing large power, by internal combustion, appears to arise mainly from the unreliability of cast iron and cast steel in withstanding serious and quick changes of temperature. Large cylinders applied to this purpose have had disappointing results in consequence of destructive cracks which have developed from this cause. It is likely, however, that engineers and metallurgists, working together, will solve this problem, and so produce castings, improved either in material or in form, which will stand the high impulses and wide variations of temperature which have to be encountered when fuel, in both liquid and gaseous forms, has to be used for the development of great power on board ships.

Growth of the Mercantile Marine of the United Kingdom.

It will be of interest here to consider the extent of the increase in British-owned ships during a period of 10 years ending in 1905. In 1895 the number of British-owned vessels was 21,003, having a gross registered tonnage of 12,992,405, of which number 12,617 were sailing ships, having a gross registered tonnage of 3,040,194. In 1905, or in 10 years, the gross registered tonnage of British-owned vessels had increased to 16,680,420, or to the extent of 28 per cent., whereas the tonnage of the sailing ships during the same period had decreased to 1,796,826, being a reduction of 41 per

cent. It will be observed from the foregoing figures that the average gross registered tonnage of steamers existing in 1895 was 1,187, whereas in 1905 it had increased to 1,415 tons.

The Development of Shipping as affecting Harbour and Dock Accommodation.

At the Engineering Conference in June the subject of further increase in the dimensions of ships was discussed in connection with the provision of a suitable margin in the design of future wet and dry docks and the improvement of harbours. No exact rules can be quoted as the outcome of this discussion by which harbour engineers can be guided in dealing with new projects, but it was made clear that from the point of view of shipowners and naval architects increased draught of water is of the highest importance to economy of propulsion and cost of sea transport.

Sir William White discussed this matter at some length in his Presidential Address in 1903, and then said that all first-class ports should have provision for ships up to 1,000 feet in length, with 100 feet breadth of entrance, and up to 35 feet depth of water.

Lord Pirrie in his valuable Note, which resulted in an interesting discussion, gave it as his opinion that our channels, our trading-dock entrances, and our graving-docks, in most cases appear to have been constructed without due regard to present requirements, and more frequently without any reference whatever to the demands of the future. Foresight, by which he meant an attempt to anticipate developments, had not, he said, been displayed by our port-authorities in these matters to anything approaching the extent one would have expected in the interests of those authorities themselves, apart from any national benefits arising therefrom. Later on in the Note he remarked that it would be difficult to estimate the extent to which shipbuilding had been stayed by the inadequate supply of docking-facilities, but certainly, in his opinion, we should have been much farther ahead to-day in the matter of large ships, had these facilities, accompanied by deeper channels, been available earlier.

With respect to future developments, Lord Pirrie's view is, that we may rest assured still larger ships will be built, and the increase will be stimulated by the provision of improved accommodation in the direction he indicated.

With regard to the alleged want of foresight on the part of port-authorities in anticipating the advent of these large ships of to-day,

it is doubtful whether even shipbuilders themselves realized, until comparatively recent times, the extent to which growth in ships is destined to develop. Many instances could be cited in the experience of engineers associated with the construction of harbours and docks, where designs have been seriously curtailed for financial considerations, and in this connection it should be borne in mind that decision in these matters mainly rests with those who control the purse-strings. Nor should it be forgotten that ships of extreme dimensions, or docks to accommodate them, will not be constructed, unless there is a reasonable prospect of a suitable return on the outlay. Although on financial grounds, therefore, our designs may be sometimes restricted, it is certainly incumbent on us that we should be mindful in framing our projects, to arrange them, whenever possible, so that the requisite additions may be made hereafter without undue modification of finished structures.

The day has evidently arrived when larger ships will be generally built, and the works and waterways in the ports frequented by such ships will have to be reconstructed and extended accordingly. Unless harbour- and dock-authorities are actively alive to this fact, it may be taken for granted that the ports over which they hold jurisdiction will fall back in importance and give place to more enterprising rivals.

Professor Biles, in the discussion on Lord Pirrie's Note, called attention to vessels of 700 feet long as compared with vessels of 500 feet, the advantage running up to 20 per cent. when the draught was increased in proportion to the length. Increase of length without increase of draught was disadvantageous. There is not very much doubt, Professor Biles rightly remarked, about the increased earning-power of large ships per ton of goods carried, but there is some doubt about the increased earning-power of large ships unless the necessary amount of goods is forthcoming. Therefore the extent of increase in the size of ships which is profitable, depends very much on the volume of trade which is behind it, which again is reacted upon by the lowness of freight which the large ship brings with it.

Many of the large liners engaged on the American and Canadian services are ships of a special class mainly adapted for passenger-service and carrying but little cargo. Although accommodation for them is required at such ports as Liverpool and Southampton, and also suitable depth at the Tyne, the Clyde, and Belfast, where they are generally built, it would be obviously unreasonable to provide for the reception of such large ships at many ports not in the first

rank. Nevertheless in all places the requirements and demands of the future, with regard to the particular class of trade carried on, should be fully kept in view, and in this last-named connection it may be advantageous, in some instances, for the dock-engineer to confer with the naval architect.

In support of the need for increased accommodation it should be mentioned that many ships, in consequence of restricted depth, cannot be laden to their maximum draughts even when cargo is available. As bearing on this point Lord Pirrie remarked that he was concerned as builder in the production of some large ships trading to London, designed with a load draught of 34 feet 3 inches, which were built as long ago as 1898, in the hope that the channel would be deepened and the port otherwise improved to an extent that would have enabled the owners to take the fullest advantage of them, but he regretted to say that it had not yet been possible to load these ships down to their marks, and although not fully laden they have often to wait for high water to go into dock.

Another phase of the question, mentioned by Dr. Elgar, is that owners who are content with their ships, state, in discussing this matter, that it is quite true larger docks and deeper water may be advantageous for a port and for special ships, but as the carrying out of such improvements would probably necessitate an increase of rates, they do not see why the class of ships that do not require the further accommodation, should bear a part of the expense in the shape of additional dues. The financial considerations operating all around this question of increased dock and waterway accommodation are therefore very complex, to say the least.

In considering the foregoing questions it is not possible to generalize, or to lay down definite and all-round rules for the guidance of engineers. Shipowners and shipbuilders appear to be unable at present to go beyond the bare statement that provision should be made for expansion in the future on a liberal scale, and so far we are agreed. The competition of the leading ports and the desire of those who control them to retain their trade, will to a large extent solve this question.

Dock and Waterway Accommodation at Home.

The Thames and its dock-entrances, viewed by the light of modern experience, are no doubt unsatisfactory. The limitation of draught of ships at low water of spring-tides may now be taken at 24 feet, but as the rise of such tides is, say, 20 feet, there is plenty of water

at *high* tide even at neaps. What is required in the Thames, however, is a greater depth in the low-water channel, in order to render the navigation independent, or nearly so, of the rise of tide, and of the consequent restricted periods for traversing the course of the river and for docking, to say nothing of fogs. If the channel could be deepened to give, say, 30 feet at low water, in accordance with the recommendations of two Commissions and as contemplated in recent Bills, as to which there would appear to be no engineering objections, much good would be done, especially if associated with deep-water lock-entrances of adequate dimensions; which improvements should enable the Thames to hold its own for a further and it is hoped a prolonged period, against home and foreign rivals.

The Thames Conservancy are already actively engaged in the formation of a channel, 30 feet deep at low water, up to Gravesend, and are obtaining powerful plant of the suction-dredger type, which will be available, at an early date, for work on the Leigh Middle Shoal.

It is gratifying to learn, from an announcement made some months since in the House of Commons, that the Government intend to introduce, during the coming Session, a Bill dealing with the Port of London, although it has not been considered desirable hitherto to make any statement with regard to the details of the Bill.

Mr. Lyster, in his Note on "Dredging in the Sea-Channels of the Mersey," introduced at the recent Conference, referred to a proposal for new docks and entrances on a large scale at Liverpool, which received the authority of Parliament during the Session of 1905–6. In this scheme it was considered necessary to make provision for vessels of 1,000 feet in length and of 40 feet draught. It is also contemplated to deepen still further the outer channel of the Mersey. No fixed measure of improvement in this last-named respect has been determined on, but after a careful survey of existing conditions, and a comparison with probable requirements, it has been decided to construct a dredger of 10,000 tons capacity provided with pumping power equivalent to about three times that of any of the existing Mersey dredgers. Since 1890 the depth on the bar has been increased from 11 feet at low water of spring-tides to 28 feet.

To illustrate the growth of the trade of Liverpool, it may be stated that in the year 1886 the ships' tonnage entering the docks was 7,350,000 tons, whilst in 1906 it was 12,755,000 tons, an increase in 20 years of 74 per cent. To arrive at the total tonnage which entered and left the docks these figures must be doubled. To

keep pace with this growth of trade the Mersey Docks and Harbour Board has, within the last 16 years of the period named, spent upwards of £8,000,000 in building new and re-constructing old docks, with the result that the docks to-day can accommodate the largest vessels afloat. It is difficult to realize that docks which were only adapted for vessels drawing 20 feet of water, are now fully available for steamers drawing 30 feet. This has been brought about by excavating below the foundations of the old walls and putting in deeper ones without disturbing the quay-surface; at the same time the floors of the docks have been lowered by powerful rock-cutting dredgers.

The Mersey authorities have also provided graving-docks of corresponding size, and in particular one having a length of 925 feet and an entrance of 94 feet in width, which is amply sufficient for the accommodation of the new Cunarders, and has been, in fact, already used by them.

At present vessels with a draught of 25 feet 6 inches navigate the Manchester Ship Canal regularly, and have a clearance of 15 inches under the keel. When the deepening operations now in progress have been completed, this canal should be capable of accommodating ships drawing 27 feet to 27 feet 6 inches. The expenditure to date on this great work, including its accessories, has been about £10,500,000.

At Southampton, which in these latter days may be, to some extent, regarded as a rival port to Liverpool, the navigable depth in the approach-channel to the docks is 30 feet at low water of spring-tides, with depths alongside the basins and wharves, which are tidal, of 18 feet to 32 feet, also at low water of springs. Extensive improvements are in progress, or will shortly be commenced, both with regard to increased depth in the fairway leading to the docks to admit of the accommodation of White Star and similar liners, and also to additional berthage, in the form of a new dock, having 35 feet in the first instance, and ultimately 40 feet alongside at low water of spring-tides. By the enterprise of the London and South Western Railway Company, who now own the docks at Southampton, two fine graving-docks have been provided there, which are adequate for the accommodation of liners of the White Star and Hamburg-America type.

The largest lock now under construction, of which I have any knowledge, is that at Newport, Monmouthshire. It will have a length of 1,000 feet between the gates, and is to be divided into two compartments of 600 feet and 400 feet respectively; the width of the entrance will be 100 feet, and the depth over the outer sill

45 feet at high water of spring-tides. At Avonmouth the lock-entrance to the new dock, in course of construction there, is also very capacious, being 875 feet in length and 100 feet in width, the graving-dock leading from the wet dock being 850 feet long and 100 feet in width at the entrance. At Portsmouth the Admiralty have decided to construct a new lock leading to the existing basins of 850 feet in length, the entrance width being 110 feet and the depth over the outer sill 46 feet 6 inches at high water of spring-tides, which should be ample for the accommodation of warships for many years to come, keeping in view extension in dimensions

Both at the Tyne and the Clyde dredging has been recently undertaken in order to provide additional depth for the accommodation of the new Cunard steamers, one of which has been built in each locality. The authorities on the Tyne are moving to obtain a depth of 30 feet at low water, and on the Clyde they are approaching the same depth; to go to any great extent beyond this might raise serious questions with regard to the reconstruction of wharves and quays, and would possibly necessitate the removal of rock-patches. On the Tyne the tendency is to provide additional berthage, when required, in the river, and to furnish the same with the newest form of hydraulic coal-shipping staithes.

THE SUEZ CANAL.

Leaving the home ports, it may be remarked that harbour facilities in the East come under an influence which is not operative in the western hemisphere or in Europe. The Suez Canal is there the key to the situation at present, and engineers, however persuasive in their advocacy of greater depths in harbours and docks, are met with the somewhat unanswerable contention that there is little gain in providing for ships which cannot navigate De Lesseps's great waterway. It should not, however, be forgotten that large cargo-carriers go to Australia by the Cape of Good Hope. It may become a question of the relative economy of the shorter mileage by the canal, used by vessels of dimensions limited by the proportions of the waterway, and the higher carrying-efficiency of large-capacity cargo-liners on the longer route, combined with saving the canal-dues, which are heavy. The engineers of Indian and eastern harbours, and the authorities they serve, must not lose sight of this alternative, and such considerations may have a beneficial influence on the policy of the administrators of the Suez Canal. It is well, therefore, that adequate provision for ships of deep draught should be fully appreciated.

I am glad to reflect that Sir Charles Hartley, who for so long a period served with distinction on the Suez Canal Commission, as well as Sir John Wolfe Barry, who succeeded the late Sir John Coode on the same Board, have been among the most active and persistent advocates of greater width and greater depth of water in the canal. It is worth recording that the original Commission of 1855–56, when considering the proposals for a canal, contemplated as a minimum width 144 feet at the bottom and 262 feet at the top—on the section between the Bitter Lakes and the Mediterranean—and that the depth of water should be 8 metres (26¼ feet). This depth was adopted, but the width at bottom was reduced to only 72 feet. The promoters need not be criticized because they failed to anticipate fully engineering developments which stimulated shipowners to avail themselves, even at heavy cost in dues, of the shortening of the sea voyage to the East by the completion of the canal.

The demand for a fuller section of waterway resulted in the appointment of the 1884 Commission, and later on of a Sub-Commission, of which the engineers I have named were members. Sir Charles Hartley has told the story admirably in his Paper on "The History of the Engineering Works of the Suez Canal." The result of the Sub-Commission's investigations was a proposal to increase the depth from 8 metres (26¼ feet) to 9 metres (29½ feet), and the width at bottom to 213 feet in straight reaches and 262 feet at the sharpest curves, between Port Said and the Bitter Lakes, and thence to Suez 246 feet in straight reaches and 262 feet at the curves. The British representatives contended in favour of a depth of 9·5 metres (31 feet) from end to end of the canal, but their proposals were not adopted. Financial considerations weighed with the Board, which induced them to decide to first deepen the canal to 8½ metres, which was accomplished in 1898, and then to proceed with making it 9 metres, which was completed in 1902. Work is now in progress to ensure a depth from end to end of 10½ metres (34 feet 5 inches), and at the same time the widths are to be made 100 metres (328 feet), and the radii of curves are to be increased to a minimum of 2,500 metres to the north of the Bitter Lakes and 3,000 metres to the south of them. It will be some years, however, before this work is accomplished, although its necessity is universally recognized.

The progress may be summed up as follows: before 1884 the maximum draught of ships permitted in the canal was 24 feet 6 inches; in 1901, 25 feet 7 inches; in 1906, 27 feet; and in 1909 it may be 30 feet. There have been corresponding improvements in widths and curves. The increased dimensions of ships using the

canal are indicative of the fact that owners are ready to avail themselves of improved facilities. When, in the future, vessels drawing 30 feet and more will be permitted to navigate the canal, such ships will undoubtedly use the waterway in increasing numbers. Many of the ports in the Far East are already adapted for vessels of this type.

Sir Charles Hartley and Sir John Wolfe Barry resigned their seats on the Commission last year, and were succeeded by Mr. A. G. Lyster and Mr. L. F. Vernon-Harcourt. The lamented death of the latter created a gap which has not yet been filled, but it may be taken for granted, that the engineering interests in the canal, from a British standpoint, will be in capable hands and well safeguarded.

The Panama Canal

It will be interesting now to consider the works in progress in connection with the future alternative route to the East by way of the Panama Canal.

In May, 1904, the Government of the United States of America, on payment of an agreed sum of $40,000,000, assumed possession of the whole estate and property which, under the direction of the French Courts, had been vested in a Company by the official into whose hands the undertaking had passed, on the failure of the original Company to complete the work. The way was thus made clear for Mr. Roosevelt to proceed with the construction of the canal under the terms of what is known in the United States as the "Spooner" Act, which was passed by Congress in June, 1902. This Act provided that the President should be authorized to construct a canal on the Panama route, and that this canal "shall be of sufficient capacity and depth to afford the convenient passage of vessels of the largest tonnage and greatest draught now in use and such as may be reasonably anticipated."

For the purposes of construction the President appointed a Canal Commission, the functions of which were to be somewhat similar in character to those exercised by a Board of Directors in this country.

So far as the alignment of the waterway was concerned, this, for the greater part of the route, was already determined by the work which had been carried out by the two French companies, which work the President was, under the terms of the "Spooner" Act, required to utilize.

The question with regard to forming the canal at sea-level, or with locks, led to much discussion both in Congress and in the American press, with the result that the President appointed an

International Board of Consulting Engineers to advise him as to the type of canal which should be constructed. This Board consisted of thirteen members, eight being American and the remaining five European engineers. Of the European members, four were nominated, respectively, by the Governments of Great Britain, France, Germany, and the Netherlands, the fifth member representing the Suez Canal Commission.

The report of the majority (as the Board differed in respect of the recommendations made) was finally signed in Brussels in January, 1906. Eight members, including all the European representatives, recommended the construction of a canal at sea-level, that is to say, a canal without locks, except at the Panama end, where, in order to avoid undue current-velocities, tidal locks were proposed. The necessity for this last-named provision was brought about in consequence of the sensible difference of tidal range at the two termini of the canal, the range in the Caribbean Sea being seldom as much as 2 feet, whilst that in the Bay of Panama sometimes reaches 20 feet. The remaining five members recommended a scheme for a locked canal, which had found advocates in the United States, but which differed from the projects previously prepared.

The sea-level proposal was objected to on account of its cost, which (including 20 per cent. for contingencies) was estimated at $247,000,000, whilst the scheme of the minority met with much hostile criticism because of, amongst other things, the great risk which must attend the passage of war-vessels, larger and heavier than the "Dreadnought," through such a flight of locks as that proposed at Gatun, in order to overcome the difference of level, of 85 feet, at that point.

Notwithstanding this criticism, the Isthmian Canal Commission, as then constituted, expressed their approval of the scheme of the minority of the Board, that approval being endorsed by the Government and by a majority in both Houses of Congress in the Session of 1906. Circumstances have apparently, however, since led the advocates of a canal at sea-level to decline to regard this decision as final, or to consider the question as closed.

With regard to the position of the work at the present time, the American Government have practically completed the operations which they undertook for the purpose of placing the isthmus in a sanitary condition, which have proved entirely successful, and are now vigorously prosecuting the excavation of the canal. The greater part of this work is being carried on in the "divide," i.e., in the 7 to 8 miles of hill country which are known as the "Culebra

Cut," and which contain 110,000,000 cubic yards out of the total of 231,000,000 cubic yards of excavation required for the completion of the sea-level canal.

In the month of August of this year the United States Government had 11,733 white men and 28,710 coloured men at work on the canal-route and on the Panama Canal Railroad (which is the property of the Government), or a total number of 40,443 men, as well as the greatest equipment of plant ever collected on one undertaking, of which a large part has been recently constructed for the canal-works.

The excavation is being prosecuted without cessation, the dredging of both approaches is in progress, other operations are being pressed forward, while the whole of the work in hand is of such a character, and is being executed in such a manner, as will render it available for either type of waterway. It may be therefore assumed that the great problem as to whether the canal will be actually formed at sea-level, or constructed with locks, is still waiting for definite solution.

The depth proposed for the canal is 40 feet, which is a generous provision, to which no exception can be taken on the ground of insufficiency.

A Few Remarks on some Foreign and Colonial Harbours.

It has been stated recently, and apparently with some justification, that the accommodation in the Port of London, both in the river and in the docks, is superior to that at Hamburg, Rotterdam, and Antwerp.

The forty-five miles of channel of the River Elbe leading up to Hamburg have now a navigable depth of 26 feet at low water, this result having been secured by an expenditure of over £700,000 on dredging. The rise of tide being only about 6 feet, the berthage accommodation provided takes the form of tidal basins, of which there are thirteen, with a depth of 26 feet at low water. The area of the basins and that part of the river available for ocean-going steamers is about 550 acres and the total length of the quayage about 19 miles.

The Port of Antwerp is between 50 and 60 miles from the mouth of the River Scheldt; the latter has many sandbanks and is very winding. Vessels drawing 16 feet can get to the port at any state of the tide, but in the port itself there is about 22 feet in the channel at low water. Spring-tides rise nearly 15 feet and neap-tides about 13 feet. The accommodation in the port

consists of river-quays and docks. The former are nearly 3½ miles in length, with a depth alongside at low water of 26 to 33 feet. There are eight docks and three basins for sea-going vessels, having a water-area of about 150 acres and quays nearly 7 miles in length; the depth over the sills of the docks is 21 feet at low water. In addition, the new intercalary docks will have a water-area of 61 acres, with quays somewhat over 1½ mile in length, and a depth sufficient to accommodate vessels drawing 30 feet. A large scheme of extension has for many years been the subject of political dispute between the State Government and the Municipality, and is still in abeyance.

The accommodation at Marseilles is provided almost completely behind the shelter of a breakwater 11,500 feet in length running parallel to the shore. There are six existing open basins having a collective area of about 320 acres, in which the depth of water varies from about 20 to 50 feet. Around the basins extensive quays and cargo-sheds are provided. Considerable new works are in progress, by which basins of about 100 acres in area, with a length of about 8,000 feet of deep-water quayage, will be added; and the port is laid out in such a manner that practically unlimited extensions can be made hereafter as required.

At the Amsterdam and North Sea Canal a new lock has been constructed 738 feet by 82 feet, and the canal itself is being deepened, so that next year it will be available for vessels drawing generally 9 metres, or 29 feet 6 inches, and, with special permission, for draughts 1 foot greater.

With regard to colonial ports, it will not be possible in the limited time available to do more than refer, in general terms, to a few of those of which I have personal knowledge. Taking first the Eastern ports approached through the Suez Canal, reference may be made to Colombo, one of the largest, if not actually the largest, artificial mercantile harbour which has been yet constructed. The enclosed or sheltered area is 660 acres, and the three protecting breakwaters are together about 1½ mile in length. The two main breakwaters on which the heaviest sea-stroke falls each consists of a superstructure of heavy concrete blocks, laid in inclined slices, with what is known as sloping bond, on a rubble mound base, the footings on the sea side being protected either by an apron of concrete in bags or by a deposit of large rubble.

The enclosed area of 660 acres has a depth almost throughout varying from 30 to 36 feet at low water, which can be increased hereafter, if necessary, by dredging, as the depth in the Suez Canal is improved. The rise of tide is 2 feet. Along the landward margin

of the harbour extensive reclamations for coal-storage and other purposes have been formed, which yield important rentals. On this margin also a graving-dock has been provided which is sufficient for the accommodation of any ship, either of the Navy or the mercantile marine, which may be expected at this port for many years to come, and which may have previously passed through the canal. A satisfactory feature which should be noted is the perfect circulation within the enclosed area, notwithstanding that the tidal range is only 2 feet. This is important, in view of the floating population engaged in or upon the margin of the harbour, and the extent to which it is occupied by the numerous and very fine passenger-steamers which frequent Australia and the Far East. Colombo has been referred to as the "Clapham Junction" of the East, as nearly all boats call there for transhipping passengers or for coaling.

The works formerly sanctioned are now drawing near completion. It is anticipated they will be carried out within the estimated expenditure, and already the revenue derived from them is sufficient to pay interest and sinking-fund on the loans issued for their construction.

A further sheltering arm, to protect the southern entrance, is about to be commenced, and the provision of berthage accommodation for passenger-steamers is also being considered. It is not improbable that extended accommodation at this port, hereafter, will take the form of a wet dock, leading out of the existing harbour, for which a suitable and comparatively inexpensive site is available on swamp lands.

From a commercial point of view Ceylon is in a very prosperous condition. It may be of interest to state that the value of tea exported from the island has risen from about £200,000 in 1885 to £4,100,000 in 1906. Within the last few years large areas have also been opened out for the cultivation of rubber, the production of which is now in its infancy, and bids fair to rival the tea industry in the near future.

The railways in Ceylon are owned and worked by the Government, and form one of the most valuable assets of the colony. The total length open for traffic is 562 miles, of which 495 are on the 5-foot 6-inch, or Indian broad gauge, and the remaining 67 on the 2-foot 6-inch gauge. An active agitation is now on foot for further extensions. The profit earned by the railways of the island during 1906 amounted to nearly 6 per cent. on their total capital cost.

The many scenic, historical, archæological and botanical attractions of Ceylon are now far more accessible than ever before, as there are well-equipped railways running to the "Buried Cities," to Kandy,

Uva, Jaffna, and Point-de-Galle, and in addition the era of motor-cars has set in, the roads being good.

The three other eastern ports to which I may be permitted to refer in brief terms are Penang, Singapore, and Hong Kong. All three are fine natural harbours, particularly the latter two. Penang may be regarded as the terminal port of the Federated Malay States Railways, and is also largely used for cargo-transhipping purposes. Berthage has recently been provided for passenger-liners, which it has been decided to extend. The traffic is mainly conducted to and from vessels lying in the harbour, where there is an ample depth of water, by means of lighters, a system which lends itself to utilization of the cheap and plentiful Chinese labour here available.

The Federated Malay States afford an example of British rule and progress which is perhaps unrivalled in any other country in recent times. In 1874 there were no roads, and the country might be described as a vast forest with a few villages scattered here and there along the banks of the rivers. At the close of 1906 there were 1,583 miles of metalled roads and 429 miles of railway opened for traffic, 120 miles being, in addition, under construction through the State of Johore; while further important extensions have been surveyed and others are in contemplation. Practically the whole cost of these railways and of the development of the country by means of roads, bridges, and buildings, has been paid out of the revenue of the States, no loans for public works being required. The principal product is tin, worked chiefly by Chinese from alluvial deposits, but of late years European companies have made great advances on the primitive methods in vogue with the Chinese miners. In 1877 tin was produced having an approximate value of £196,000. In 1906 the production was valued at £8,500,000. The production of rubber has, in late years, occupied the chief attention of the agriculturists, the called-up capital of the companies interested now amounting to more than £2,000,000.

Singapore, which has been termed the gate of the East, for through it must pass most of the eastern traffic, possesses one of the finest natural harbours in the world. It is a great entrepôt for the interchange of commerce, and on visiting it for the first time one cannot fail to be impressed with the vastness of the trade which is carried on and the extent of the shipping engaged therein.

Recently the Government have acquired the property of the Tanjong Pagar Dock Company, a growing and successful private undertaking, owning a considerable length of deep-water wharfage in the harbour of Singapore, where ocean liners and cargo-steamers are berthed for coaling and transhipment purposes, and

important works and industries are carried on. In lieu of the former company a Dock Board has been established, on lines somewhat corresponding with the Mersey Docks and Harbour Board and the Tyne Commissioners, which will control this important undertaking in future.

The wharves owned by the Tanjong Pagar Company were constructed with timber, the life of which, mainly in consequence of the extreme activity, in these waters, of the teredo, was only 5 to 6 years. To avoid the expense and inconvenience attending such frequent renewals, the new Board has decided to replace its temporary wharves, which extend about a mile in length, by permanent structures, the latter having 33 feet alongside at low water for the full length stated, with the means of increasing that depth to 35 feet when required hereafter.

Other extensive works are about to be commenced by the Board, including a wet dock of 24½ acres and a graving-dock 846 feet long with 100 feet breadth of entrance, the depth over the sill being 34 feet at high water of spring-tides. The Government of the Straits Settlements has also in hand the construction of a basin of 270 acres area and a quay of nearly 1 mile in length, for the accommodation of intercolonial and coasting steamers.

It will be seen from the foregoing that the authorities at Singapore are proceeding on comprehensive lines and are making suitable provision for extensive developments in the future.

Hong Kong is the port of call for steamers *en route* to China and Japan. It was rumoured recently that considerable shoaling was occurring there, but happily on careful investigation this proved to be baseless. The traffic with vessels lying out in the harbour is carried on by lighters which are berthed at small jetties extending from the "Praya" or dwarf quay-wall. My first connection with Hong Kong was after the typhoon of 1874, and in rebuilding portions of the Praya which were damaged, Chinese labour was employed under European foremen. In those days there were no suitable cranes available in the colony; and, until proper plant could be procured, the granite ashlars and copings were replaced in Chinese fashion, as far as handling was concerned, by means of rope girdles placed under the stones, with longitudinal and cross bamboos inserted therein, until a sufficient number of Chinese shoulders could be applied to carry the stone forward and place it in position.

At Kowloon, on the opposite side of the harbour, berthage for liners is available and also a fairly large graving-dock; works likewise exist there, where repairs can be effected and small craft built. The

railway to Canton, now in course of construction, starts from this place.

I may be permitted to allude here to the increased rapidity with which structures of iron, and more particularly of steel, perish in tropical waters, as compared with the life of such structures at home. It has been my experience that the finer the quality of the material, such as steel or the better classes of iron, the greater the rate of corrosion and deterioration. It may be observed that "between wind and water," i.e., at and near low-water level, it is not good policy to be continually scraping and re-coating structures of iron in a sea-way in the tropics. The proper and natural protection for such works is afforded by the marine growths and accumulations of shell-fish which gather around and adhere to the structure, thus forming a coating of lime spread over the iron, and securing it, as far as practicable, from the corroding influence of the sea-water.

Coming now to the South African harbours, the three main ports of the Colony are Cape Town, Port Elizabeth and East London. Farther up the coast there is Durban in Natal and Delagoa Bay in Portuguese territory. Since the conclusion of the South African War the traffic at the Cape ports has fallen off considerably, but it is hoped only temporarily, cargoes for the Transvaal and the Orange River Colony being dealt with in an increased degree at Durban, but more particularly at Delagoa Bay.

The reason for the traffic in connection with the Transvaal and the Orange River Colony having been recently diverted, to some extent, from the Cape ports, is due to the more favourable railway carriage from Durban and Delagoa Bay, but especially from the latter, the distance from Delagoa Bay to Johannesburg being very much less than from either East London or Port Elizabeth. It would appear, therefore, that the Cape Colony, and Natal also in a less degree, will, in the future, have largely to depend on the internal development of their own resources for the traffic of their ports and railways.

At Cape Town there is ample accommodation for present requirements in the form of deep-water sheltered berthage, and a fine installation of electric cranes and labour-saving appliances. At Port Elizabeth there is no sheltering harbour at present; vessels lie out in the Roads, and communication with the shore in landing and shipping cargo is carried on by means of lighters, berthed alongside iron jetties, furnished with hydraulic cranes. I am not aware that any finer iron jetties than those at Port Elizabeth have ever been erected. One of them is nearly 1,500 feet in length,

and has a berthage width of 105 feet. It may be of interest to state that the first of these jetties was erected in 1880 and that the ironwork of the piles, girders, and bracing is in a satisfactory condition.

I propose to touch on the ports of East London and Durban later on when referring to bar harbours.

Engineering Work in West Africa.

I should like here to allude briefly to the West African railways and harbours, as an indication of what is being done in opening out, by engineering works, colonies comparatively little known a few years since.

The actual construction of the railway in the colony of Sierra Leone was begun in 1896 and completed in 1905, for a total length of 222 miles. The main line from Freetown to Baiima has been constructed for commercial purposes. It taps extensive palm-oil forests and serves a large and industrious population. The works on the railway comprise numerous steel viaducts and some long bridges over rivers in the interior. The gauge is 2 feet 6 inches. To assist in the development of the country, and to act as feeders to the railway, roads, and in some instances branch tramways, are in course of construction.

Early in 1898 work was begun upon a short railway in the Gold Coast Colony from Sekondi, on the seaboard, to Tarkwa, the centre of the gold-mining industry, where an important banket reef had been worked for some years. Owing to the impossibility of conveying the heavy machinery required for gold-mining to Tarkwa, the industry had previously met with only moderate success. The railway now enables the heaviest machinery to be transported. Early in 1900 it was decided to continue this railway to Kumasi, the capital of Ashanti, not only to assist the progress of the country in mining and agriculture, but also for political reasons in view of the frequent disturbances among the Ashantis. This work was carried out amidst great difficulty, due to bad climate, dense forests, scarcity of labour, and the outbreak of war with the Ashantis, and was completed in September, 1903. Surveys have also been made for other lines in the Gold Coast Colony.

With regard to Nigeria, the construction of a railway from Lagos, the capital of Southern Nigeria, was commenced in March, 1896. This line was carried out in sections as far as Ibadan, which was reached in December, 1900. After a considerable pause, work was

restarted in November, 1904, and is now being actively proceeded with as far as Ilorin, a distance of 250 miles from the coast. This railway, which is likely to become the main trunk line of Nigeria, serves numerous towns having large populations.

The general result of the works which I have briefly described has been to terminate barbarous practices, such as slave-dealing, human sacrifices and other atrocities, to suppress inter-tribal warfare and the consequent decimation of the population, to put an end to native revolts and uprisings, and to open up the country to trade by enabling hitherto untapped forest-produce to reach the coast, and European manufactured goods to be imported into the interior. In the case of the Gold Coast profitable mining has been made possible, and various forms of agriculture have been introduced over a great part of West Africa. The most important step in this direction has been taken in Southern Nigeria, where large tracts, hitherto waste land, have been opened up for cotton-growing. Ginneries have been erected at many places, and it is possible that cotton from Nigeria may to a great extent take the place of American cotton in the Lancashire markets.

At present the Lagos railway is not in direct communication with ocean-going steamers, but works have been recently commenced with a view to open out the entrance to Lagos Harbour, which, if successful, should result in great benefit to the future commercial interests of Southern Nigeria. The conditions which prevail at Lagos resemble, in a marked degree, those which formerly existed at Durban on the Natal coast, where, as I have previously stated, a fine harbour, adapted for the reception of the largest liners, has now developed. I see no reason why similar results should not attend the pump-dredging operations just commenced at the entrance to Lagos, associated as they will be with the training of the currents into and out of the harbour by the construction of moles. In this connection it may be observed that at Lagos there is a fine backwater—an extremely important feature in aiding and maintaining by scour the work done by the dredgers.

On the Gold Coast also, what may prove to be important harbour undertakings have been commenced recently. The West African coast at the present moment is practically devoid of any efficient harbour-accommodation, which is now rendered more than ever necessary in consequence of the opening up of the country by railways in the manner described.

Some Points in Connection with the Design and Construction of Harbours.

Before a design for a harbour or dock can be efficiently prepared, it is absolutely necessary there should be a thorough investigation of the physical conditions of the site. This examination should have special reference to exposure, the set and velocities of the currents, the possibility of shoaling consequent upon the proximity of accumulations of sand or shingle, the nature and depth of the shelter required and its extent; the character of the strata to be dealt with and of the materials and local labour available; and many other points, each of which demands careful and minute investigation.

Considerable variation in the height of waves, the forces which they exert, and the depth to which their action extends, is of course naturally to be met with in the practice of a harbour-engineer. Taking two or three instances in illustration, it may be remarked that at Dover a sea of greater height than 15 feet to 18 feet from crest to trough has not been recorded there since the new harbour-works were commenced in 1893; whilst at the mouth of the Tyne, in connection with the north-pier works, waves of 35 feet to 40 feet from crest to trough have been observed. At Peterhead, where a harbour of refuge is being constructed by the Admiralty, there are records of waves which closely approximate to 40 feet in height from crest to trough.

The depth to which wave-action extends is much greater than was formerly believed to be the case. At Peterhead breakwater, for instance, on the occurrence of a severe storm in 1898, blocks weighing upwards of 41 tons were displaced at a level of nearly 37 feet below low water of spring-tides, the rise of such tides being 11 feet. A section of the breakwater, weighing 3,300 tons, was also slued bodily 2 inches, without the blockwork being dislocated. Calculations showed that to effect such movement a wave-force of fully 2 tons per square foot must have been exerted simultaneously over the area in question. At Colombo, during the carrying out of the south-west breakwater, a length of wall 28 feet in width, founded at 20 feet below low water, the rise of tide being 2 feet, was slued inward by sea-action to the extent of 15 inches at the outer end, the portion of the work affected being 150 feet in length; landward of this point no movement occurred. The blocks were subsequently lifted and reset on true lines.

The north pier at the entrance to the Tyne was commenced in 1854, in accordance with the best knowledge then available, and

is 2,960 feet in length. The inner portion was founded on a rubble base at 2 feet below low water. As the work advanced seaward, it was considered desirable to carry down the foundations to a greater depth, so that at the outer termination of the pier they were placed at a level of 27 feet below low water, the footings along the top of the mound being protected by an apron of concrete blocks each 41 tons in weight. Notwithstanding this considerable increase in the depth of the foundations and the means adopted for their protection, great difficulties were experienced in maintaining the under-water portion of the pier on the sea side, particularly towards the outer end. For several winters blocks were drawn out which had to be made good during the succeeding summer, until at length the increased extent of the disturbance rendered it impossible to make good the damage during the following fine season. Consequently, on the return of winter a serious breach occurred, which has resulted in the rebuilding of the outer portion of the pier for a length of 1,500 feet.

I allude to this circumstance to show that, although the designs for the work were prepared in accordance with the best experience of the time, nevertheless the information available at that date with regard to such questions, based on actual construction, was extremely limited.

With regard to the exceptional depths to which wave-disturbance extends, the late Sir James Douglass once mentioned at a meeting here that lobster-creels, off the Land's End, lying in 20 to 30 fathoms, had been found to be filled with sand and shingle on their withdrawal after a heavy gale, some of the stones weighing as much as 1 lb.; thus showing that in that position sea-action had extended to the depth named. I may observe that off the coast of Peterhead and Fraserburgh there have been similar experiences.

Sir James Douglass also gave, at the same meeting, a remarkable instance of coarse sand having been found after a gale, on the external gallery of the Bishop Rock Lighthouse off Scilly at a height of 120 feet, the depth of water in the vicinity of the rock being 25 fathoms; thereby showing that the sea-bed had been disturbed at that depth, this being the only source from which the sand could have been obtained.

Although the movements in these last-named great depths are associated with exposed positions and refer to comparatively small material, they indicate that in some cases disturbances of the

sea-bed occur considerably below the level at which artificial sheltering works would be founded; disturbances which, unless satisfactorily provided against, might give rise to subsequent shoaling within the enclosed areas, especially where they act in connection with currents of appreciable velocities.

With reference to waves passing through an entrance into a harbour and the extent of their reduction by reason of expansion within the sheltered area, the calculated result obtained by formula should, in my experience, be considered in conjunction with the disturbance due to the wind-wave generated within the sheltered area itself. More particularly is this necessary in the case of large harbours, where the agitation caused by wind "lop," irrespective of that due to seas passing through the entrance, becomes a material factor for consideration. Even in enclosed wet docks of large extent, during gales a considerable wave-disturbance is experienced at the leeward ends, and the same action is also observed in reservoirs and other artificial areas of impounded water.

Since pump-dredging was initiated in Holland about 1880, considerable advances have been made in the construction of craft specially adapted for this class of work. Bar harbours, which before the introduction of this system of dredging were only partially successful, with reference to the works executed at their entrances for scour and training purposes, have now, in consequence of the aid afforded by pump-dredging, been satisfactorily developed for commerce. Two instances may be cited in illustration, namely the Port of Durban on the Natal coast and East London on the South African coast. Probably no bar entrances existed of a less favourable character than at these two ports before their successful treatment was commenced, first by the construction of moles and training works, and secondly by pump-dredging. Especially is this the case at Durban, where the physical conditions are more favourable than at East London, mail-steamers from home now regularly entering and leaving that port.

Another satisfactory example of the application of this system of pump-dredging is the great work which is being carried on at the mouth of the Mersey, to which I have already referred.

The introduction of Portland cement and in later years its extended use in harbour-construction, coupled with the development and perfection of plant and appliances now employed in the carrying out of sea-works, have, to a considerable extent, revolutionized all previous methods of procedure, and have undoubtedly tended to rapidity of construction, economy in cost, and the attainment

of a minimum risk of damage during execution and subsequent thereto.

Before the introduction of Portland cement, lias lime was the material principally used in mortar and concrete for sea-works. In consequence of the time required for its setting it was not a satisfactory material, especially in a sea-way, where it is important that early setting should be effected, to a sufficient extent to prevent the subsequent disturbance of the work by wave-action.

In 1756, when Smeaton was looking about for the most suitable mortar to be used in the building of the Eddystone Lighthouse, he demonstrated the fact that limestone containing clay possessed, when burnt and ground, the property of hardening under water, although the importance of this discovery was not explained scientifically until many years later. Between 1756 and 1824, numerous patents were obtained for the preparation of various cements, more particularly partaking of the character of Roman cement. In the last-named year, however, a patent was granted to John Aspdin, a bricklayer of Leeds, for his invention of "an improvement in the means of producing an artificial stone." Aspdin gave the name of Portland cement to the material made under his specification, because of its supposed resemblance, when set, to the well-known building stone quarried at Portland on the Dorsetshire coast.

Aspdin may therefore be regarded as the inventor of Portland cement. It is probable that although he knew little or nothing of chemistry, and was guided only by empirical rules, he was able, by virtue of his long experience, to produce a cement of a fairly reliable character. In any case, he kept his process a close secret, allowing no one but the workmen to enter his premises, and himself taking part in the loading of each kiln.

Two years after the registration of Aspdin's patent, General Sir C. W. Pasley commenced his experiments and researches at Chatham Dockyard, apparently not at that time being aware of the existence of Aspdin's cement. In 1830 he succeeded in producing a very good article from Medway clay and the chalk found in the neighbourhood.

As early as 1828 Brunel obtained cement from Aspdin's works at Wakefield, to be used in the construction of the Thames Tunnel, the material at that time costing from 20*s.* to 22*s.* per cask, in addition to the carriage to London, or about five times the price of the much improved cement available to-day.

The English manufacturer was for many years severely handi-

capped in his endeavours to improve the production of Portland cement by the custom which existed of each engineer drawing up his own specification, a requirement being sometimes mentioned in one clause which was rendered impossible of fulfilment by the stipulations of another. This difficulty has now been largely overcome by the adoption of the British Standard Specification for Portland cement, which was issued in December, 1904. This specification is being widely used, and has given general satisfaction alike to the consumer and the manufacturer, although, as a result of experience, it has been found advisable to revise it in certain details, the modified specification having been published in June of this year.

Although continuous experiments have been made with regard to the setting qualities and other characteristics of Portland cement for very many years, it is not a little surprising that, even now, certain irregularities in the behaviour of this material, as prepared to-day, are not absolutely and satisfactorily understood: as to these further experiments are being conducted, under the auspices of the committee associated with the preparation of the standard specification. As an indication of the extensive growth in the use of this excellent and highly important material, it is believed that the world's production of Portland cement, which during the year 1886 was 2,500,000 tons, has now risen to 15,000,000 tons per annum.

With reference to the employment of cement concrete in blocks or in mass for sea-works in the tideway, experience is generally in favour of the former. Sound work has been unquestionably produced by the use of mass concrete, but it is believed that failures have been more numerous in consequence of the adoption of the material in this form than where concrete has been used in blocks. Where breakwater or pier-works of a sufficiently extensive character have to be constructed, and the cost of heavy special plant would be justified, it will generally be found that the most economical and expeditious mode of procedure lies in the use of concrete blocks up to, say, 40 or 50, or even 60 tons in weight. Such blocks, when laid in place, would not be liable to disturbance, except under special conditions of exposure, nor would concrete in this form, when fully set, or nearly so, before the blocks were placed in position, be subject to deteriorating influences, consequent on the infiltration of sea-water into green material.

Generally, it will be found, where the sea-exposure is not too great to admit of the employment of temporary stagings with Goliath cranes travelling thereon, that such a system is to be preferred, not

only on the ground of rapidity of construction, but also as producing sounder work than the over-end method of setting which is associated with the use of travelling Titan cranes running on the structure itself as formed. With the last-named system, where horizontal-coursed blocks are adopted, the work is brought up from the foundations in short lengths, and consequently is subject to settlements and open cracks, corresponding with the addition of each section, unless the blocks are laid in slices, with "sloping bond," which is not always desirable. With the long "scar" or working end, however, which is obtainable where staging is adopted, such cracks and settlements are to a large extent avoided. Moreover, with the staging method of construction, the preparation of foundations, block-setting above and below water, and other operations, can be carried out simultaneously, or practically so; whereas with the Titan system progress is mostly restricted to one process at a time. In very exposed positions, such as at Peterhead, the use of temporary staging is not admissible, and there the Titan or over-end system had to be resorted to.

Fine examples of rubble-mound breakwaters are to be found at Portland, on the south coast, and at Cape Town, with the design and execution of which my late esteemed chief, Sir John Coode, was so intimately identified. Works of this description, in consequence of the rolling of the rubble on the seaward slopes by wave-action, require feeding with new material to compensate for the attrition of the stone and the wastage caused thereby. In most cases it is important to deposit the rubble in moles of this character from a staging, so that the mound may be readily replenished with new material from time to time, as the slopes are drawn down by the sea, until the normal angles of repose have been produced. Works of this description are best adapted for positions where there is a small rise of tide, and a consequent minimum area of exposed sea-slope. At Portland spring-tides rise less than 7 feet, at Table Bay 5 feet, and at Colombo, where there is also another successful instance of a rubble-mound breakwater, 2 feet.

With regard to under-water construction, hitherto it has been taken that 70 feet is the maximum depth in which divers can work with economical results or sustained effort. Where works have to be carried on at greater depths, men employed for any length of time under such circumstances, either in the dress or in bells, are liable to a species of "caisson" disease, partaking of the nature of cramp or paralysis. My friend, Mr. Moir, who supervised the construction of the Hudson River and Blackwall Tunnels on behalf of Messrs. Pearson, the contractors for those works, experienced the

same difficulty with the men engaged there when under air-pressure; and with a view to mitigate the extent and effects of the disease, provided, for the first time on those works, chambers for the reception of patients, where, under the influence of reduced pressure, intermediate between the intensified atmosphere in which they had been employed and the normal condition at the surface, they were gradually brought to a state of recovery.

It has been long observed in tunnelling under air-pressure, that when two cross diaphragms with their necessary air-locks are in use in a tunnel during its construction, the men are much freer from caisson-disease than when only one diaphragm is employed. Recent experiments by the Admiralty, under the direction of Professor Haldane, on deep diving in the dress by Navy divers, seem to afford a reason for this fact, which, as pointed out by him, appears to be due to the lowering of the pressure in steps or stages, rather than by allowing a continuous and regular drop of gauge-pressure per unit of time.

By adopting stage decompression, accompanied by a very slow rising of the diver to the surface, some hitherto unworkable depths have been recently reached, the divers remaining in these depths for 30 minutes at a time, and the complete ascent being ultimately made without any observed damage to the men employed. I understand that on more than one occasion a depth of 210 feet has been attained. The experiments have not yet been carried sufficiently far to show that men in large numbers could be found who could stand this pressure for any length of time, and be able to do hard manual work, but it is possible that these experiments may lead to developments of an important character in subaqueous working.

As an indication of the increased rapidity with which sea-structures may be carried on, under modern methods, as compared with former progress on similar works, I may observe that at the original Admiralty Pier at Dover, the average rate of prolongation of that work, between 1847 and 1871, was just over 90 feet per annum, whereas during the extension of the pier, in connection with the new naval harbour, it has been 450 feet, and at the south or island breakwater, where special efforts were made, 1,500 feet per annum. The works in connection with the naval harbour aggregate about 2½ miles in length. The quantity of water required to fill the harbour during an ordinary spring-tide, which passes through the entrances, is 22 million cubic yards.

Reinforced Concrete as a Material.

In the Harbour Section at the recent Engineering Conference Notes were read on reinforced concrete as a material for marine structures. In those Notes and the discussion which followed their reading much information was given with regard to piers, wharves, jetties, and other similar works, which have been actually carried out in this comparatively new material. The general outcome of the discussion appeared to show that reinforced concrete has now been used to a sufficient extent, and with such satisfactory results, as to justify its more extended employment in the future. It should, however, be borne in mind, that in each of the two works to which reference was more particularly made in the Notes which were read at the Conference, the reinforced concrete structure itself was protected from the chafing and impact of steamers and craft lying alongside, by the provision of timber-piled fendering.

With regard to the more extended use of reinforced concrete, it does not appear to have been employed hitherto in breakwater construction. Where natural conditions are suitable, however, it may be possible to provide shells or pontoons of this material, to be constructed, say, on the shore, launched, and towed into position to form sections of a breakwater or quay-wall, to be subsequently filled with concrete. A work embodying this principle has, I believe, been carried out with satisfactory results at Heyst, in Belgium, in connection with a canal leading to Bruges, but in that case the pontoon blocks were constructed of ordinary concrete, and their walls were therefore of much greater thickness than would be necessary were reinforced concrete adopted. For the successful application of this mode of construction, however, it would be necessary that there should be periods of smooth water of sufficient duration to enable the floating sections to be conveniently and safely manipulated and sunk in position, and also for the subsequent filling of them with concrete-in-mass.

Floating Docks.

Considerable development has occurred in recent years in the provision of floating docks. Docks of this character are actually in existence, or are under construction, which are adapted for dealing with the largest ships of war and the most capacious liners. Our own Navy has a floating dock at Bermuda capable of lifting warships of up to 16,500 tons dead weight, and I believe the Navy of the United States possesses docks of even greater lifting-

power. On the commercial side, I understand a floating dock is being constructed capable of lifting ships of up to 36,000 tons dead weight, thereby possessing sufficient power and dimensions to deal with vessels of about the size of the new Cunarders.

It is claimed for floating docks, and of course rightly so, that they are distinctly cheaper than graving-docks and can be built much more rapidly. It has also been stated by Mr. Lyonel Clark, who has done so much for this system of construction, that there are docks of this character, at present in existence, which have been working for upwards of 40 years. Those which are self-docking, or can be docked, and which have been carefully looked after, are, he says, still in good condition. Others which have never been docked, but allowed to slowly deteriorate, are now practically worn out.

To compare such docks with graving-docks, the conditions of the site and the nature of the accommodation to be provided, will, on careful investigation, generally indicate in which direction the balance of advantage lies. The two types are not rivals, and there is no question of one supplanting the other: they are both efficient, each possessing its own special features; and it is for the engineer to examine carefully these points as circumstances arise, and to decide which of the two is the more suitable for his purpose.

Lighthouses and Signalling.

With regard to the lighting of our coasts, the most important improvement which has been effected during recent years has been the gradual substitution, which is now practically accomplished, of incandescent-oil burners for the concentric-wick burners which were previously used. These burners, which vaporize oil under pressure, have proved very efficient in service, as they not only vastly augment the intensity of the lights, but are much more economical in the consumption of oil than the concentric burners were. As an example of the increased economy and intensity which are obtained by the adoption of this improved system, the case of the Eddystone may be cited. With the former six-wick burners, the consumption of oil there was 2,500 gallons per annum, which has been reduced to 1,200 gallons per annum by the use of incandescent burners; and with regard to intensity, whilst the full power of the beam issuing from the biform dioptric apparatus was 79,000 candles, with the incandescent-oil burners it has been increased more than three-and-a-half times, to 292,000 candles.

No new electric lights have been established on the English coast

for many years past, as the committee of scientific and other experts, who investigated at South Foreland in 1884–5 the question of the relative merits of electricity, gas, and oil, for lighthouse illumination, reported that oil is the most suitable and economical illuminant for ordinary use, and that electricity should be adopted only for important headlands or landfalls where a very intense light is needed. At the Lizard the two fixed electric lights previously there have been superseded by one quick-flashing electric light, and a similar light has been substituted for the half-minute flashing electric light formerly at St. Catherine's, Isle of Wight. These new lights are among the most powerful in existence, their reflection being frequently visible from the deck of a vessel at a distance ranging from 60 to 70 miles.

With regard to the relative advantages of gas, oil, and electricity during thick weather, this matter was carefully inquired into by the committee already referred to, and they reported, with respect to the last-named illuminant, that, notwithstanding its greater ratio of diminution in intensity as distance increases, its vastly higher initial power renders it visible at a greater distance in hazy weather than the highest powers tried in gas or oil.

Recently at some of the electric lighthouses on the English coast "flame" carbons have been introduced, the chemical treatment of which has the effect of tinting the beam of light and rendering it much more penetrative, under unfavourable conditions of the atmosphere, than the white electric beam. The conversion of the lights to the incandescent-oil system is being effected on the French coasts, as well as in other European countries and in our Colonies.

Safety of life and property at sea is to us, as a maritime nation, a matter of the highest importance. The attention which has been devoted to the consideration of the loading and equipment of ships in recent years has produced remarkable results in that direction. Of the causes of loss still remaining, fog is undoubtedly one of the most potent. No one can have been at sea in foggy weather, without having realized the helplessness and dangers which the presence of fog imposes on navigation. For some years past this problem has been studied carefully, and considerable advances have taken place. Great efforts have been made to improve sound-signals in air and to diminish the risks of their imperfect penetration and failure.

At the shore and floating-light stations under the jurisdiction of the Trinity House the old caloric engines formerly employed as the motive power for air-compression in connection with fog-signals

have been largely superseded by more powerful oil-engines. Means have also been adopted at several shore stations for the better distribution of the sound, and in many other respects important improvements have been effected.

In connection with the emission of coast and floating fog-signals, certain peculiar phenomena are experienced, due doubtless to strata or zones of the atmosphere being more saturated and therefore more obstructive to the development of sound than adjacent sections; consequently difficulties are sometimes encountered with regard to sufficient penetration, under such special conditions, of sounds emitted by the ordinary siren or fog-signal.

More recently a system of submarine sound-signalling has been devised. Persistent effort on the part of the promoters of the scheme has led to successful results. The system now available originated in the United States. It has been practically tested and approved by naval authorities in that country and in Canada, by the British Admiralty and by the Trinity House. The source of sound in this system is usually a submarine bell of special form and tone. Mechanical apparatus is adopted for working the clapper, and for varying the interval between strokes. The receiving apparatus consists of microphones placed in tanks filled with sea-water, fitted on each side of the bow, inside the ship, a few feet below the water-line. These microphones are connected by telephonic apparatus, with ear-pieces that can be placed in any position which may be chosen—by preference in the chart-house or navigating-station. It has been proved in actual service that by means of this apparatus it is possible, in fogs, to fix accurately the positions of lightships, or stations, at which submarine bells have been fitted, in ships moving at high speeds, at distances of 5 to 7 miles, and in some instances at even greater lengths.

The system has, I understand, been applied in the approaches to New York, Liverpool, Cherbourg and other ports. Although only 3 or 4 years have elapsed since it was introduced practically, more than one hundred of the principal steamships of the world, aggregating about 1,400,000 tons, now carry the receiving apparatus. It is gratifying to gather from a recent announcement that the Board of Trade has given the necessary financial sanction to enable the Trinity House to establish submarine-bell signals from several of the Corporation's light-vessels.

Dock- and Wharf-Equipment.

With reference to the general question of the equipment of docks and wharves, with either hydraulic or electric plant, it appears that, although hydraulic power undoubtedly possesses advantages for coal-hoists, the actuating of dock-gates, for lifting sluices—and not improbably also in manipulating swing-bridges—nevertheless for the ordinary crane-equipment of docks and quays, electric power has demonstrated its equality with hydraulic power, and possibly, in some instances, its superiority. The electric-lighting of quays and warehouses is also an important matter, and provides a welcome load when the power-demand is at its lowest.

Concerning the relative efficiency of cranes and capstans, electric and hydraulic, much has been said. The result of extended experience has established the fact that a modern hydraulic crane or capstan, working at a maximum load, absorbs practically the same energy as its electric equivalent, but for loads less than the maximum the electric system shows a proportionate economy.

It is a subject for regret that as yet there are so few figures available as to the actual cost of working a dock entirely equipped with electric machinery. In view, however, of the increase in the installations of electric plant in connection with docks and harbours, it may be reasonably assumed that the experience hitherto obtained with this class of machinery has been of a nature to warrant extensions.

Submarine Cables.

In the conduct of the commerce of the world the use of submarine cables plays an important part. Cables still exist and are being worked which were laid nearly 40 years ago. The life of a cable depends chiefly upon the nature of the bottom. A level bottom of ooze in deep water is, of course, favourable to long life, but even in such cases it has been found that corrosion of the iron sheathing wires has been set up by chemical action, due probably to the presence of mineral ores. In shallow water, in addition to corrosion from this cause, there is chafing due to the continual motion caused by currents and by heavy waves, the effect of which is felt at considerable depths. In such waters the bottom is frequently of sand, shells, rock, or coral. Boring insects may attack the cable and penetrate the insulator at depths of several hundred

fathoms, but protection has for some few years past been obtained by covering the core with thin brass tape. In shallow tropical waters it is not uncommon for cables to be damaged by fish-bites, and sharks have been known to cause such damage even in depths of 300 fathoms, the animal being identified by the tooth left in the cable and recovered when the latter was lifted. The life of shallow-water cables, for the above among other reasons, is less than that of those in deep water, necessitating a systematic policy of partial renewals, which are, of course, carried out at the positions which show the principal deterioration. Unfortunately, the occurrence of faults or breaks is the only notice that is given of such damage.

Repairs are, of course, more easily effected in shallow water than in the deep sea, where cables now lie in nearly 4,000 fathoms, or, say, 4½ miles of depth. In such cases repairs are expensive and difficult, success being largely dependent upon the continuance of sufficiently smooth weather.

It is, of course, to the advantage of ocean-telegraph companies that the forwarding of messages should be encouraged and confirmed in every way. The establishment of feeders from outlying districts is, therefore, of importance. Where the traffic is not sufficient to justify the capital expenditure for cables, the companies look to wireless telegraphy to fill the gap. For this purpose it has undoubtedly a large field, and at the present moment a considerable installation is being prepared to connect various islands in the Azores archipelago, which hitherto have been without telegraphic connection.

The distance between London and New York is 4,000 miles. The time for transmission of ordinary cable-messages between these two cities averages 10 to 12 minutes. There is another class of traffic, however, namely that between the Stock Exchanges of London and New York. In consequence of the difference in time—5 hours—there remains in London a very short part of the business day for the exchange of messages; therefore there is need for an instantaneous service, and messages between the Stock Exchanges of London and New York are, I understand, thus transmitted in 30 seconds. Frequently an inquiry is sent to New York and a reply received in London in 2 minutes. In short, a stream of messages between the two cities can be kept up by one of the companies, at the rate of ten a minute, including delivery; hence a broker in London can exchange with his correspondent in New York as many as three hundred messages in an afternoon, and there are several brokers who do this every day.

It is satisfactory to note that large reductions have been made in the tariffs during past years, the rates in force at the present time showing a diminution of nearly 50 per cent., on some lines, as compared with those which prevailed in 1893.

The growth of submarine cables in the last 10 years is represented by the following figures:—

1897.	Government cables . . .	19,263	nautical miles.
„	Private companies	145,154	„ „
	Total . . .	164,417	„ „
1907.	Government cables . . .	44,988	nautical miles.
„	Private companies	216,116	„ „
	Total . . .	261,104	„ „

I have endeavoured to obtain particulars with regard to the chafing of submarine cables in comparatively shallow waters, with a view to throw light upon this important subject from the point of view of harbour-construction, as to the depths to which wave-action extends and the effects produced; but regret that I have not been able to procure reliable information bearing thereon which would be useful in this sense.

Wireless Telegraphy.

The development of the new art of wireless telegraphy has proceeded at a rapid pace. Although it is but a few years ago that the method of signalling by Hertzian waves was brought to a practical stage by Mr. Marconi, there were at the beginning of the present year in connection with this system no fewer than 195 stations, either wholly or partially devoted to commercial work, in all parts of the world, and 147 stations provided for purely naval, military, and lighthouse purposes.

The range has gradually increased up to 1,500 miles, and has recently culminated in Mr. Marconi's great achievement, in enabling messages to be conveyed from this country to America and Canada, independently of ocean cables.

Summary.

In the foregoing remarks I have referred, in too imperfect terms, to certain branches of Engineering which are associated with the conduct of over-sea traffic, and therefore have an intimate and important bearing on our maritime commerce. In what directions may we anticipate developments and improvements in the future? In the ships of our mercantile marine we may with certainty look for expansion with regard to both dimensions and numbers. Although our yearly percentage of increase may not be relatively as high as in the case of one or two other nations, nevertheless the annual additional tonnage to our mercantile fleet is still far in advance, and we hope and believe will continue so.

Again, we are fully entitled, in the light of recent events, to anticipate in the immediate future, further and possibly great developments in steam propulsion with turbines, either employed alone or associated with reciprocating engines. Then, there is the extended use of oil for raising steam, or directly in internal-combustion engines.

With regard to harbours, docks, and waterways, we are fully alive to the importance of making due and adequate provision for larger and deeper-draught ships, in the designs to be prepared for new works, and also where harbours and docks exist of inadequate dimensions for present requirements. It must not be forgotten, however, that although, all around, ships of increased capacity will have to be accommodated, it is at comparatively few ports that the very largest vessels will be found, at all events for a long time to come; and therefore it may be preferable, in many cases, in view of the exigencies of finance, to proceed tentatively, and to extend hereafter, rather than, in the first instance, to carry out works of undue magnitude, which may remain unremunerative for considerable periods.

As to the actual construction of sea-works, the arrangement of their design so that their execution may entail, as far as possible, repetitions of the same process, with the use of heavy masses and the generous application of suitable plant, may be usually expected to produce satisfactory and economical results, as far as the structures themselves are concerned. There are, however, associated with such works many considerations not infrequently highly important and sometimes complex—in regard to which it is not possible to lay down general rules; and therefore the conditions apper-

taining to each special case should largely determine the procedure to be adopted. Under the most favourable circumstances the carrying out of such works entails anxiety, which, however, is generally equalled by the special interest awakened.

There are many other points in connection with colonial and tropical ports on which I have not touched, such as the necessity for an adequate supply of pure water and the adoption of satisfactory sanitary arrangements, and again, the extension of railways, which sometimes precedes but is invariably associated with harbour and dock developments. Sufficient has been said, however, to indicate to the young engineer that there is still a vast amount of professional work to be done, particularly in our Empire beyond the seas and in undeveloped foreign countries.

Mr. J. C. HAWKSHAW, Past-President, in moving "That the best thanks of The Institution be accorded to the President for his Address and that he be asked to permit it to be printed in the Minutes of Proceedings," remarked that probably no living engineer had travelled over so large a portion of the world for the purpose of considering important questions of harbour-improvement as Sir William Matthews. Of the subject which he had chosen for his Address he might with truth be said to be a master. But, interesting as the portion of his Address which he had read that night had been, Mr. Hawkshaw felt sure that the portions which had had to be omitted were of equal interest, and the members would look forward with pleasure to reading the complete Address in the Minutes of Proceedings.

Sir ALEXANDER R. BINNIE, Past-President, observed that it was his pleasure and his privilege to second the motion. He was sure the Members must all have listened not only with interest but with considerable instruction to the Address the President had delivered; and although it would ill become him to speak in terms of laudation, he felt that so good a commencement of a term of office was a very good augury of the success which would attend Sir William Matthews in the Chair.

The resolution was carried by acclamation.

The PRESIDENT thanked the members for the manner in which they had listened to his remarks. In working out the subject and preparing his Address he had found that it ran to much greater length than he had anticipated, and therefore he had only been able to read it in an abridged form. No doubt in doing that he had earned the gratitude of the members; but, however that might be,

those who felt inclined to continue their course with him to the home ports and into the Colonial seas might be able to obtain from the complete Address a few more particulars with regard to the special ports with which he had had to deal.

The PRESIDENT then presented the Howard Quinquennial Prize, and the Telford, Watt, George Stephenson, and James Forrest medals; and the other awards made by the Council in respect of Session 1906–7 were announced.

A reception was held subsequently in the Library.

12 November, 1907.

Sir WILLIAM MATTHEWS, K.C.M.G., President, in the Chair.

(*Paper No. 3688.*)

"The Extension, Widening, and Strengthening of Folkestone Pier."

By Hugh Torrance Ker, M. Inst. C.E.

The works which form the subject of this Paper were begun in 1897 and completed in 1905. Before proceeding to describe them, reference should be made to the vast accumulation of shingle which has resulted from harbour and foreshore protection-works carried out at Folkestone during the last hundred years.

Historical.

The first harbour-works of any importance were those authorized by an Act of 1807 for the protection of the craft frequenting the port from the strong southerly winds. They consisted of a pier composed of open rock-work, laid as sloping blocks, which remain to this day as originally set, and form the wall of the existing inner harbour. The projection of the pier seaward cut off the travel of the shingle from west to east, and caused an accretion, which in course of time became so large that there was a renewal of the eastward drift round the end of the pier, tending to block the entrance to the harbour.

In 1818 an Act of Parliament was obtained for constructing a scouring-basin or inner harbour, and these works were duly carried out, but without the desired effect of keeping the shingle away from the harbour-mouth. The breakwater was extended seaward with a view to maintain a serviceable depth at the entrance, but the growth of the shingle on the weather side continued steadily until once more the normal travel to the east was resumed. The same process

appears to have continued during most of the nineteenth century. Each overflow has been met by a further extension of the pier, which in its turn was banked up with shingle as before.

The result is that an area of approximately 26 acres has been reclaimed from the sea, and the end of the pier recently constructed, while only 1,400 feet from the foreshore on its western side, is 2,300 feet from the eastern shore, which is practically the original coast-line of 100 years ago. The area so reclaimed has been of inestimable value in the development of the port. Without it there would have been no space available for the railway-station, goods-yards, warehouses, stores, customs-offices and other appurtenances of a trading-harbour. These have all been erected on part of the area of reclaimed land, and in addition a terrace of houses has been built, with a large public garden in front of it, where formerly the sea must have been deep enough for small craft.

Since about 1870 the shingle has ceased to gather in anything like the same degree as before that date, no doubt owing to the extension of the groyning to the westward, and no apprehension of further extension-works being required on account of shingle-travel is now entertained. Indeed, as will be described later, an increase in the accumulation is encouraged by the erection of groynes immediately to the west of the root of the pier.

In 1897 the old pier was in a very dilapidated condition, and as its upkeep during the execution of the new works presents some features of interest, its construction will be briefly described.

The old pier was originally a roughly-built mound 540 feet long and 30 feet wide, formed of loose rocks of about 2 tons weight deposited from a staging of old rails driven as piles and cross braced. The top of the mound did not reach higher than about half-tide level, while the top of the staging was 10 feet above high water of ordinary spring-tides, and formed the deck upon which the traffic for the cross-channel service was conducted. The depth of water in which this pier was built was about 6 feet at the inner and 20 feet at the outer end below low water of ordinary spring-tides. The rise of tide at Folkestone is 20 feet at springs.

About the year 1870 the mound was raised in a solid rubble wall to deck-level, and the pier was widened by building on each side of it a wall, 15 feet wide, of sloping concrete blocks up to 3 feet below high water of ordinary spring-tides. Outside of these walls were driven Memel timber double piles 10 feet between centres, which were protected by elm rubbing-pieces. The piles rose to 13 feet above high water of ordinary spring-tides, and supported cross timbers which carried the deck over the lower landings. The latter

were formed by paving the levelled tops of the sloping blocks with bricks on edge. The sloping blocks were laid very irregularly, and indeed no one system appears to have been carried through. The blocks were not taken down to a secure foundation, and as settlement occurred the lower blocks dropped away from those above, leaving gaping joints in which the action of the sea took full effect. The concrete of which the blocks were made was in many instances deplorably bad, and was rapidly crumbled away by the waves. The result was that huge cavities were formed in the underwater portions of the sides and end of the pier, and the whole work was threatened with destruction.

The method which had been adopted for repairing the damage and filling up the cavities acted very successfully as a temporary measure, and was continued during the progress of the new work, until the old pier was entirely encased by the work about to be described. The method of repairing the old pier was as follows. Whenever a cavity was found to exist, a diver fixed short lengths (about 6 feet) of old rails vertically against the sides of the Memel piles on each side of the cavity. These vertical rails formed stops for the short rails, which were next inserted horizontally, thus forming a kind of panel behind which concrete in small bags, such as the diver could handle with ease, was packed into the hole. The rail panelling was raised as the filling proceeded until the cavity was completely blocked. This method was adopted all round the pier, so that for an average of about 20 feet from the bottom the pier was practically faced with old rails. It was manifestly no more than a temporary measure of repair depending entirely on the life of the Memel piles, which were sooner or later eaten away by the teredo.

The existence of the old pier had almost reached its limit when the new works were begun, and it was with the utmost difficulty that it was kept up until the new works were raised to make it secure. A few of the old Memel piles which carried the weight of the deck were perfectly sound above high water, but below low water they had disappeared entirely owing to the ravages of the teredo. Other piles were bulged at half-tide level to an alarming extent by the spreading of the side walls of sloping blocks, and they were only kept from breaking away altogether, and letting down the blocks, by the insertion of tie-rods 2½ inches in diameter, extending through the pier from side to side, and tying them together.

At the root of the old pier on the west side a solid wall was impossible owing to the line of the pier forming with the stone groyne a re-entering angle facing the worst exposure. The

passenger-station which was built on this site had therefore been carried on a series of buttresses about 30 feet by 18 feet, and 18 feet apart, thus leaving vents for the seas to expend themselves. These buttresses were seriously undermined and in a state of collapse, bulging seaward and breaking up owing to defective cement. Their destruction was prevented by putting them in splints formed of old rails fixed vertically and hooped with horizontal rails fished together. Temporary protection to their foundations was also afforded by depositing from a barge an apron of 20-ton blocks.

The New Works.

Traffic-Requirements.—The increasing trade between Folkestone and Boulogne was very inadequately served by the accommodation on the old pier, so that irrespective of the demand for works of considerable extent to make the old pier secure, an extension and improvement in the accommodation was urgently required. In considering the design for the new works the class of traffic and the mode of working had to receive careful attention.

The passenger-service in 1897 carried 111,000 persons with their baggage. In 1903 the number was increased to 211,000, and during 1905 there was a further increase to 257,000. To accommodate this important service berths had to be provided for the packet-steamers, to be available at all states of the tide and in all weathers. Each berth required facilities, which have been provided by special cranes, for the handling of the baggage of the passengers with the despatch which has been one of the features of the service on this route for many years. The railway-traffic had also to be accommodated in such a manner that the passenger-trains could discharge or embark passengers and baggage conveniently at all the berths.

In addition to these the very important goods-traffic passing through Folkestone had to be provided for. This traffic is divided under two main heads, namely, ordinary merchandise, of which the chief items are wine and wool, and "grande vitesse," which consists mostly of perishable articles such as fruit and flowers, and also Lyons silks and other articles, which have to be forwarded with the utmost possible despatch. The total quantity of goods dealt with annually is about 100,000 tons, and their character is shown by the fact that the value of the goods passing through Folkestone annually is about £12,500,000. Besides the above-mentioned classes of goods there is an extensive traffic in horses, for the convenient shipment of which provision had to be made.

It will be seen therefore that the works required had to be capable of serving not only as a breakwater on an exposed coast, but also as a pier with considerable quay-space for the convenient handling and despatch of passengers and goods, and for the contingent station-platforms with their offices, waiting-, and other rooms; and also for the examination of baggage and goods by the customs-officials.

Nature of the New Works.—The new works comprise:—

1. An extension of the old pier by 900 lineal feet, of which 300 feet are in direct continuation of the old work and 600 feet are inclined at an angle of 36° (to the E.S.E.), for the purpose of giving shelter from the prevailing S.W. winds. (Figs. 1 and 2, Plate 1.)

2. The protection of the west face of the old pier by a solid wall carried down to a secure foundation. (Fig. 4.)

3. The protection of the root of the old pier by a solid wall carried on steel and concrete cylinders sunk into the lower greensand, and protected by a wave-breaker of 20-ton pell mell blocks. (Figs. 2 and 7.)

4. The renewal of the east face of the old pier by providing greenheart sheet-piling over its whole length, and a new deck throughout.

5. The provision of four new and renewal of two old berths with upper and lower landings, stairways, and horse-slipways. (Figs. 2 and 6.)

6. The construction of a protecting parapet, with a public promenade on top, on the west side of the pier throughout its whole length. (Fig. 3.)

7. Station-platforms and the necessary station-accommodation. (Fig. 2.)

8. Main lines and sidings on the pier, controlled by electric signalling.

9. Special cranes on overhead travellers, capable of dealing with vessels on both sides of the pier. (Fig. 6.)

10. The electric lighting of the pier throughout, including all the lower landings, and the provision of a gas- and water-supply, and drainage.

11. The erection of a lighthouse and fog-signal house at the end of the pier, and the establishment therein of a double flashing white light and reed fog-horn installation, according to the requirements of Trinity House.

12. The protection of the foreshore immediately to the west of the pier by a timber breastwork, groyning, and rock walls. (Fig. 2.)

Character of Site.—The borings which were taken over the site of the proposed works showed that under a cover of sand and gravel,

varying in depth from 6 to 12 feet, the lower greensand was reached at a reasonable depth for foundations, and made it possible for an upright-wall type of pier to be adopted with its foundations directly on the greensand.

General Design of Pier.

The extension-works were so urgently demanded, not only for the benefit of the traffic but for the maintenance of the end of the old pier, that as soon as the general type of cross section had been decided upon the works were begun.

The typical cross section (Fig. 5, Plate 1) shows the general method of construction adopted throughout. The pier consists of 6-to-1 Portland-cement concrete blocks, whose maximum weight was 20 tons and the average about 16 tons. The blocks were all laid as headers, and so arranged as to break bond. The face-blocks were keyed together by 4-to-1 circular joggles 10 inches in diameter and 2 feet deep. These joggles were deposited as mass concrete in bags of selected canvas. At 2 feet 9 inches above low water a 6-inch set-off on each side was provided for compensating any irregularities which might appear when the divers brought up the blockwork above low water. Above the low-water course both sides of the pier were faced with granite built into the blocks in the yard, as described further on.

The blocks above water were moulded with grout-cavities to ensure the grout obtaining a better grip of the blocks than it would do on a plain surface. Provision was also made in the face-blocks, where necessary, for receiving the upright timbers carrying the decks over the landings.

The general plan of the pier and the type of cross section having been decided the work-yard was laid out.

Materials.

Cement.—The cement was received by rail in consignments of 100 tons and stored in a shed divided into thirteen bins, each capable of holding 100 tons, with room for turning by hand. The cement was required to pass the usual tests as regards tensile strength, fineness of grinding, specific gravity, etc., and a sample taken from every third consignment was chemically analysed. No cement was issued for use until it had been aërated by storing it for a month, during which time it was turned over five times. In addition to the usual tests referred to, the cement was required to stand a boiling-test

before being issued for use. It was considered that the boiling-test formed a convenient method of deciding whether the cement was sufficiently aërated. The usual practice was to make the ordinary tests as soon as the cement was bulked in its bin, and to prepare a set of briquettes for tensile tests to be broken at 7, 14, 21 and 28 days. As soon as the 7-day briquettes were broken they were plunged into boiling water and boiled for 3 hours. If, as in many instances was the case, the briquettes showed no signs of swelling or softening, the cement was issued for use after it had received the requisite five turns without further boiling-tests being made. If, on the other hand, the briquettes were swollen or softened after being boiled, a new set of three briquettes was made after the first turning, boiled as before when 7 days old, and the result noted. If these were not quite satisfactory, a further test was made after the next turning, and so on.

In every case where the briquettes from the fresh cement were found to be swollen or softened after boiling it was noticed that a gradual improvement took place as the cement was aërated. Generally the specified five turns were sufficient to produce the desired result, i.e., a briquette which when 7 days old would stand the boiling-test; but it was sometimes necessary to aerate for a longer period and to turn the cement several times more in order to obtain this. The largest number of turns ever required was ten, and in this instance it was interesting to notice, when the briquettes were laid out in their order, what a distinct improvement there was in their condition. The briquettes from the fresh cement were much swollen and cracked all over, while those from the last turning could have been put back into the mould in which they were made, and were extremely hard.

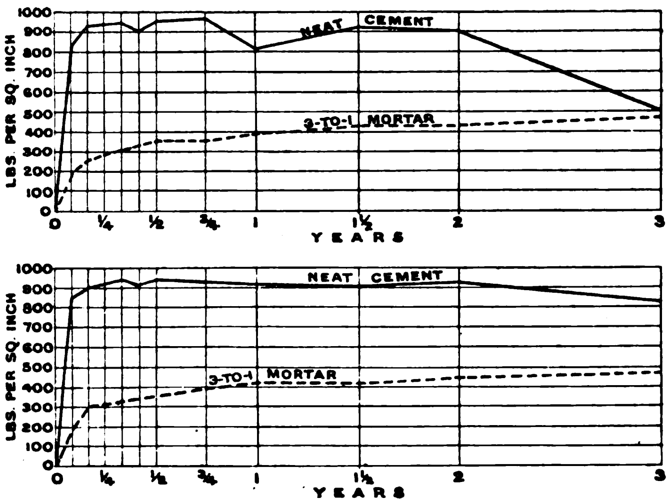
One effect of aerating cement is to increase its volume, and as cement is bought by weight and used by its bulk measurement the increase of the latter is valuable as a set-off against the cost of turning.

One of the bins was specially prepared for measuring the increase of bulk due to aeration, and some interesting particulars were obtained. It was ascertained that the increase varied greatly with different consignments, which in other respects, such as fineness, chemical analysis, etc., were very similar. The general average increase on the Folkestone works was such that the cost of turning was defrayed by the increase of bulk up to the sixth turn. In some consignments there was practically no increase after the third turn, while in one particular case the cement maintained a rate of increase more than sufficient to pay for turning up to the twenty-

second turn. The effect of aeration as regards the tensile strength of the cement was very beneficial. The time for setting was extended from 5 minutes to 25 minutes, and the results of the tests of briquettes broken at 2 days, 4 days, or 7 days old were not so high as those made when the cement was fresh; but the long-period tests (extending up to 3 years) were perfectly satisfactory, inasmuch as the neat-cement tests gave a tensile strength of 700 lbs. per square inch, and the 3-to-1 mortar tests nearly 500 lbs. per square inch.

While the fineness of a cement is an important factor tending towards soundness there is the unfortunate consequence that a

Figs. 8.



TENSILE STRENGTH OF CEMENT ADULTERATED WITH GYPSUM.

cement very finely ground is rendered quicker-setting than one of a coarser nature. Frequently during very hot weather it was found that the concrete made by the mixer for blocks had begun to set before it reached the moulds, and considerable difficulty was experienced in producing satisfactory blocks under these conditions.

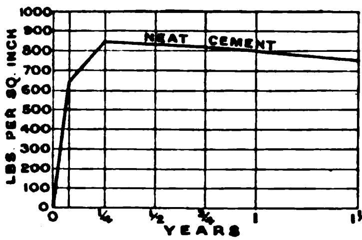
The control of the setting-properties of a quick-setting cement may be successfully effected by the well-known practice of adding a small percentage of gypsum to the cement; but whether this practice may be adopted without fear of producing unsound work is a serious problem the solution of which the following experiments were intended to advance.

Two consignments of very slow-setting cement, known to contain a slight adulteration of gypsum, were carefully tested over a period extending to 3 years. The briquettes were all kept in sea-water, maintained at a uniform temperature and changed once a week. They all passed satisfactorily through the boiling-test above described and had a high tensile strength, which, however, suffered a severe diminution in the last year of the test. This is shown in *Figs. 8*.

It will be noted that although the neat-cement briquettes lost in tensile strength towards the close of the period, the 3-to-1 mortar briquettes continued to gain steadily.

In addition to the above, a sample of cement taken from a consignment stated by the analyst to be of normal condition was adulterated with 1 per cent. of gypsum in the testing-room, and tested in a similar manner. In this case the tests were not carried further than 18 months, but the results obtained confirm generally those above referred to. There was a rapid increase of tensile strength attaining a maximum of 850 lbs. per square inch at the end of 3 months, and a steady decline thereafter to 755 lbs. per square inch at 18 months. *Fig. 9* shows the results of these tests.

Fig. 9.



TENSILE STRENGTH OF CEMENT ADULTERATED WITH GYPSUM.

The period over which the foregoing experiments, and others on the same quest, have extended may be considered too short for a conclusion to be drawn that gypsum, when added in just sufficient quantity to control the setting properties of a cement, does or does not produce any ultimate injury to the structure in which such adulterated cement is used.

The one inference which may be drawn with safety from the result of the above tests is that any proposal to interfere with the natural action of a pure cement should be treated with the utmost caution.

It may be noted in connection with this that when gypsum in the form of plaster of Paris is added, in quantities above 10 per cent., to a pure cement, not only is the mixture incapable of producing sound work, but it is rendered much more quick-setting than the pure cement by itself.

There can be no doubt that some absolutely sure method of

controlling the setting-properties of a very finely-ground cement without affecting the soundness of the work would be of inestimable value, and a question which engineers may have to consider is whether a very finely-ground cement, with its setting-properties regulated by the addition of gypsum so as to be under perfect control, is to be preferred to the same very finely-ground cement unadulterated with any controlling ingredient, although it may set too quickly to permit of its being properly gauged and worked under all conditions for the production of satisfactory work.

In the first of these alternatives there is the possibility that after a long period of years the gypsum may have a deleterious effect at present concealed, and in the other there is the danger of continuing to work concrete or mortar in which the cement has begun to set, the men so employed generally flooding the mixture with water to make it work freely, with the result that the cement is killed, and the concrete or mortar has no cohesion whatever.

There are so many cases where it is imperative that the time of setting of cement should be slow enough to allow of the gauging and working being under perfect control, that any efforts for attaining this end with absolute security as to the permanent soundness of the work should be eagerly welcomed.

Block-Making and Stacking.—The blocks were all 6 feet by 4 feet 3 inches in section, and varied only as regards length, except in the case of landings and the roundhead where special quoin and radiating blocks were required. The moulds were set up with core-bars to form the lewis-holes, and boxes in which the tee head of the bar could turn, each containing two pieces of beech upon which the pull of the lewis was taken in lifting. The long blocks were provided with a single tie-rod through the middle to prevent the sides from bulging, and indeed to keep them very slightly concave for work above water. The box was then coated with soap-oil, and the floor sprinkled with sand to break the air-seal and to prevent the block from sticking to the floor.

The concrete was mixed in a Messent mixer (capacity 1 cubic yard), driven by a portable steam-engine which also worked the ballast- and cement-hoists. The mixers, of which there were two, were each capable of turning out 120 cubic yards per day. The materials for the concrete were all brought by rail, the shingle from Dungeness, the sand and gravel from Higham. The proportions of the shingle to the sand and gravel varied from day to day, and even from hour to hour, so that without screening the sand from the gravel no hard-and-fast rule could be laid down. After repeated experiments in the early days of the work it was left to the inspector and the fore-

man of the mixer to vary the proportions as they considered from their experience to be advisable.

The section of concrete exposed when a block had to be cut through proved that the concrete was of excellent quality, being exceedingly hard and tough. There were no interstices whatever, and there was not an excess of sand.

The sand, gravel, and shingle were run to the foot of the mixer house in Decauville wagons of $\frac{1}{3}$ cubic yard capacity. Their contents were arranged in the proper proportion as regards sand to gravel and shingle, and three wagons formed a charge for the mixer. They were then raised by friction-hoists to the floor above the mixer, and tipped into a hopper, where the cement was added. The cement was filled into sacks in the shed by measuring out $2\frac{1}{4}$ cubic feet for each. The sacks were run to the cement-hoist and lifted to the top floor of the mixer-house, whence they were emptied, two to each charge of the mixer, down a shoot to the hopper containing the sand, gravel, and shingle. After mixing, the concrete was run along level staging in Decauville wagons, and tipped into the moulds, where it was worked on the faces by two men. The core-bars were generally drawn the day after the mould was filled, the boxes loosened, and the tie-rods turned round in the concrete. The block was not lifted till it was 7 days old, when it was placed in the stack to mature for other 4 weeks before it was set.

For the face-blocks above water, moulds were set up with two sides and one end, the other end being closed with a wall of granite blocks built in 2-to-1 mortar. The blocks were laid in Flemish bond with stretchers 12 inches wide on bed and headers of 20 inches, so that a thoroughly good bond was obtained with the concrete. The block face was finished with tuck-pointing in neat cement. Mooring-ring anchors were also built into the blocks for special situations. The blocks were lifted by a travelling Goliath crane of 58 feet 6 inches span, which traversed the whole length of the block-making floor and stacking-ground. The Goliath was provided with an overhanging arm so that the blocks might be carried through the side frame and deposited on trollies for transmission to the pier.

Methods of Construction.

Staging.—The staging from which the construction of the pier was carried on was in two bays each of 50 feet 6 inches span in cross section. The longitudinal bays were 40 feet in span. The staging was carried by 18-inch by 18-inch Oregon pine piles driven into the greensand in clusters of four for the outside dolphins, and clusters

of six for the central dolphins. The piles of the dolphins were braced together with old rails down to low water.

The dolphins carried lattice-girders, 3 feet 4 inches deep, longitudinally. One pair of girders lay over each line of dolphins, those in the centre-line being stronger than the side girders. Across each pair of girders, sleepers 14 inches by 14 inches were placed 2 feet apart between centres, and on these was carried a longitudinal waybeam with the traveller rail. The height of the rail above high water of ordinary spring-tides was 20 feet. At each bay there was a bracing girder, and the outermost three bays were strengthened by diagonal ties of steel-wire rope. The staging proved very stiff and never suffered damage from rough seas, though these during severe gales sometimes broke through the girders.

The pile-engines were simple cantilevers with a clear span of 47 feet 6 inches, so that while resting on one set of girders they might drive all the piles for the next dolphin ahead, and the leaders be run out clear of those piles to enable the caps to be fixed. As soon as a dolphin was ready to receive the girders, the outer end of the cantilever was propped up on the new dolphin and tackle was lowered from the middle of the bay to pick up the girders, which were floated out on a barge. The girders were lifted by means of the pile-driving winch, swung round, and landed on the caps ready to receive them; the road was then made on the top, and the pile-engine advanced for the next bay.

The time taken to erect one bay complete was generally about a fortnight, most of which was taken up in pile-driving. The winches which were used could give about forty blows per minute with a 30-cwt. monkey, but the driving of an 18-inch square pile into greensand proved very laborious. As the staging was advanced in front, the landward bays were removed and brought into use in turn. There was considerable wastage in the timber owing to the teredo, and it was found that 18 months in the sea was as long as a pile could be used with safety. There were ten bays of staging altogether, but as two of these were generally taken up with the pile-engines and another generally undergoing removal, the available working-length was only 280 feet, which was hardly sufficient to maintain a scar-end as long as was to be desired.

There were four travelling cranes running upon the staging—two on each half section. The more advanced of each pair was capable of dealing with a working-load of 30 tons and was used for working the diving-bell. The landward travellers had each a working-load of 20 tons, and were used mainly for block-setting. Both sets of cranes were specially arranged for working a $1\frac{3}{4}$-cubic yard grab.

They were also fitted with differential gear for working round the curve, of which the least radius was 212 feet. The sharpness of the curve rendered it necessary to remove the flanges of the centre wheels of the carriages of the traveller, so that they acted as rollers only, while the end wheels kept the traveller on the rail.

The amount of cover to be removed before the greensand was reached was considerable—about 12 feet deep over a long length. The grabs were of very little use when they reached the greensand, and the diving-bells were then brought into use. These bells, of which there was one working in each half section, were of wrought iron, and were 13 feet by 10 feet by 6 feet, weighing, with the cast-iron ballast inside, 26 tons out of water and about 4 tons when immersed. Four men worked in each bell, filling the greensand into a skip hung in the middle of the bell.

The bells were worked with their length longitudinally, so as to cover the beds for two tiers of blocks.

Block-Setting.—When the beds were ready to receive the foundation blocks, the bells were set to work further in advance and the block-setting travellers were brought forward to set the blocks, which were placed in position by helmeted divers. A proper line was maintained by surveying each double tier of blocks as it was set. It was found early in the work that the heavy plumb-bobs, weighing 90 lbs., were not to be depended upon, owing to the tidal current running round the scar-end.

The method of locating exactly the position of the last blocks set (which at the end of the pier were 80 feet below the staging, and at high tide in 60 feet of water) was as follows. A line was fixed to a plug in one of the lewis-holes, or in one of the joints, and strained taut from the gantry of the traveller. It was then plumbed carefully and the position surveyed exactly on the top of the staging. The necessary instructions could then be given to the diver regarding any small alterations in line, or regarding the next blocks to be set. The bell was always run over the top of foundation-blocks to ensure that they were perfectly level, and to remove any small projecting corners. The level was also checked by steel tape from the top of the staging. At the very beginning of the block-setting the divers were much handicapped by the irregularity of the work they were called upon to build. This was caused not only by special quoin-blocks forming the return end of the inner west landing, but by the proximity of the old pier, into which blocks had to be fitted. A ½-inch-scale model was prepared of the end of the old pier, and on it were shown the staging and the new work. The blocks in the model were built up as the work

under water proceeded, and the divers obtained great benefit by seeing in miniature exactly what they had to do before they went under water.

The surface of the lower greensand fell gradually seaward, so that in order to maintain the desired minimum depth of 2 feet in the greensand it was necessary to form steps in the foundation. Where these occurred great care had to be exercised in order that the then outermost tier of foundation-blocks should not be undermined. The depth of the step was limited to 1 foot 9 inches, which was run down on a steep slope, a corresponding splay being taken off the bottom edge of the foundation-block which formed the step.

It was found that although every care was exercised to keep close joints in the foundation-course a certain amount of "creeping" took place. It is not meant that the blocks moved after they were deposited, but that, owing to the difficulty of keeping joints as close in the foundation-course as in the upper courses, the blocks of the foundation were found to be in advance of their proper position. This, if unchecked, would ultimately have destroyed the bond, and a limit of creep of 12 inches was therefore established, the length of the blocks being 6 feet on the face. When the limit was reached a tier of blocks was cut or specially cast to bring the next tier to its normal position. The blocks nearest the centre-line of the pier crept quicker than those on the face, no doubt owing to the shuttering necessary for the mass concrete in the centre of the pier.

Low-Water Course.—When the blockwork had been raised to the low-water course, the top of which was 2 feet 9 inches above low water of ordinary spring-tides, preparations were made for bedding the upper blocks in 2-to-1 mortar. The blocks of the low-water course were first of all adjusted for level and all irregularities were chipped off. The joints were then carefully caulked with old bagging and canvas to prevent the mortar beds from being washed out as the tide rose. The beds were then spread for the face-blocks, and block-setting proceeded. The mortar beds were protected from the wash of the sea by pointing roughly all round the bed-joints and up the vertical joints with a quick-setting mixture composed of about equal quantities of plaster of Paris and Portland cement. This mixture set hard in about 3 minutes and formed an effective protection. During the ebb next following that on which blocks had been set, the grouting of the vertical joints was proceeded with. As the tide receded, care was taken to notice whether the vertical joints and lewis-holes were standing full of water. If they were found to be so, then it could be safely

inferred that the bed-joints were perfectly sound and uninjured. If, on the other hand, the vertical joints and lewis-holes were empty, it meant that there was a leakage somewhere, and that the bedding was imperfect. In many of the latter instances it was found practicable to make good the bed by stopping the leak and grouting up the bed through the lewis-holes, in which a close-fitting plunger was worked to drive the grout into all the crevices.

Before beginning to set blocks the temporary pointing of cement and plaster of Paris was entirely removed from the joints of the blocks already set. The face-joints were all raked out, and tuck-pointed in neat cement so as to be in keeping with the rest of the work.

Roundhead.—The blocks in the roundhead were arranged so as to produce the best bond possible without an excessive number of special blocks. The size of the special blocks was of course limited by the strength of the travellers. The tiers of blocks with joints at right angles to the centre-line of the pier were carried forward as far as possible, the face-blocks and the next internal ones only being made with radiating joints.

The method adopted for setting out the head under water produced excellent work, and may be briefly described. The block which contained the centre of the roundhead had moulded into its top surface a piece of wood 15 inches square, with 1-inch holes bored at 3-inch centres longitudinally and horizontally. Into one of these holes a screwed eye-bolt was fixed with a line attached. The line was carried to the traveller and strained taut, very carefully plumbed and surveyed. If the eye-bolt was found to be out of the central position, instructions could then be given to the diver to move it so many holes one way or the other. When the pin was in its correct position a trammel-rod with a looped end and weighted was swung round from the centre pin by the divers, who could thus check the position of each face block as it was set and make whatever alterations were necessary. In this way the roundhead was brought up in perfect line, although the depth of water was 60 feet at high water of ordinary spring-tides, and the current ran at about 3 knots.

The largest number of blocks set by two travellers in one day was seventy-six. The total number of blocks used in the pier-works was 14,294, representing a bulk of 133,506 cubic yards.

Mass Concrete.—Owing to the staging being in two bays in cross section, it was impossible to carry the blockwork through from face to face unless by adopting a straight joint along the centre of the pier. To obviate this objection, and to ensure a proper bond, every

alternate tier only was a through course, the intermediate ones not meeting by a space of about 6 feet. There was thus a space in every second course of about 6 feet by 6 feet by 4 feet 3 inches to be filled with mass concrete. There were also irregular spaces to be filled where the centre dolphins of the staging occurred at every 40 feet. The concrete-in-mass deposited below water was 4-to-1 concrete, deposited in open-topped skips with tripping doors. It was found on examination that this concrete set as hard as rock, and that the only loss was on its surface where the water had washed away the cement while the helmeted divers were levelling it. The general practice was to bring up the mass concrete about 2 inches higher than the finished level and trim off by use of the bell after it had set. Where an open end occurred temporarily, a stop was formed for the mass concrete by using mass concrete in small bags of about 1½ to 2 cubic feet capacity.

An opinion was expressed that concrete mixed dry, filled into the bags, and deposited under water, would be as sound as that mixed wet and deposited at once. The following experiment was made to test this. A bag of concrete was made according to each method, and the two were put into the sea and left for a month. When they were broken it was impossible to notice any difference—both were set extremely hard. It is very doubtful, however, if the method of depositing dry concrete under water would be satisfactory with bags larger than those referred to, namely, about 1½ to 2 cubic feet. The probability is that with larger bags the concrete nearest the outside would set before the water had time to percolate to the centre of the mass, much in the same way as bags of cement which have been dropped into water are frequently found to have a hard shell several inches thick, enclosing a core of dry cement. The total quantity of concrete-in-mass deposited in the pier was 41,682 cubic yards.

Berths.

Each berth was provided with two landings, one at 10 feet above high water, for use from high water down to half tide, and the other at 3 feet below high water, for use from half tide down to low water (Fig. 4, Plate 1).

Wherever landings occurred, chases were provided in the granite face for receiving the greenheart "piles" which were to support the deck. The chases were 15 inches wide and 7 inches deep and extended down to the top of the low-water course.

The lewis-bolts, which hold the piles to the wall by means of

fender-bands, were secured by rich mortar gauged rather stiffly and well rammed in with a thin lath. This method was preferred to grouting; but it can be adopted only when the hole bored to receive the bolt is large enough to allow of effective ramming. This will always be the case when lewis-bolts are made with bottle-shaped ends, as was done in this instance. The lower landings at the main berths are formed 13 feet below the coping for a width of 20 feet, and paved with granite. They are provided with stairways and horse-slips to deck-level.

The "piles" carrying the deck are of double 15-inch by 15-inch greenheart timbers, resting upon the low-water course and secured by 5-inch by $1\frac{1}{8}$-inch galvanized fender-bands fastened to the wall by the lewis-bolts mentioned above. Upon these timbers rest the girders which carry the deck. There was a scarcity of headroom and the depth of the girders was limited to 15 inches. They are of the box type, 2 feet 4 inches wide over the flanges, so that the web-plates enclose the piles and form a secure fastening. The girders are anchored back into the blockwork to withstand the heavy strains at the tops of the piles from vessels moored at high tide. The rails over the landings are flat-bottomed, and are carried on longitudinal way-beams. The deck between the rails is of 5-inch by 4-inch pitch-pine carried on transoms.

The framing of the decks for the west landings is of pitch-pine. These landings are exposed to very severe wave-strokes, so that a close deck of pitch-pine was out of the question. Cast-iron grids 4 inches deep with a 1-inch mesh were provided, and have acted satisfactorily except next to the wall at the outer west landing, where the seas burst upwards with great force during gales. Several grid-plates with the 1-inch mesh were carried away during a storm. They were replaced by plates with the mesh enlarged to $1\frac{1}{4}$ inch which have not been disturbed. All the bolts on this landing had to be provided with lock-nuts, as the constant vibration during stormy weather loosened the single nuts.

Parapet and Platform.

On the west side of the pier throughout its whole length is built a parapet for the protection of the passenger-trains from the prevailing west winds (Fig. 3, Plate 1). The parapet is 11 feet thick and 26 feet above high water of ordinary spring-tides It was composed of concrete-in-mass, faced on both sides with granite blocks built in situ. The platform, which also extends the whole length of the pier, is 15 feet wide on the inner arm and 7 feet

6 inches on the outer or canted arm, where there is less width over all. The face of the platform is built of Kentish rag coped with York paving, and the surface is laid with $\frac{3}{4}$-inch Seyssell asphalt. Underneath the platform are conduits laid for the electric cables, signal- and telegraph-wires. The platform is covered by a roof of steel troughing supported over the wide platform by cast-iron columns. The troughing was filled and covered with concrete finished off with $\frac{3}{4}$ inch of Seyssell asphalt, which is also carried over the width of the parapet, thus forming a public promenade which is a source of appreciable revenue. The parapet-promenade is protected on the sea side by a granite coping-stone 3 feet wide and 2 feet 9 inches high, which carries one leg of the baggage-crane referred to hereafter. The inner side of the promenade is protected by a strong hand-railing. The surface of the promenade slopes seaward at a gradient of 1 in 30 for drainage purposes, and outlets are cut through the granite about every 50 feet.

At each west landing two openings through the parapet, each 9 feet wide, are provided for access to and from the trains and boats, the promenade and baggage-crane above being carried on an elliptical arch in granite. During westerly gales it is necessary to close these openings for the protection of the working-surface of the pier, and strong sliding storm-doors have been provided for the purpose. The doors are formed of steel troughing with a channel-bar frame, and are hung on rollers from a strong bar fastened above the opening. The bottom rib of the door has a T-bar running in a slot cut in the granite for the purpose of preventing the door from being moved out of position by the sea, while at the top of the door stops are formed to prevent the door from being lifted. The channel-bars forming the frame of the door are turned towards the wall, and are filled with oak, standing proud of the iron, for the purpose of reducing the jar on the door and wall during rough weather. The doors are hung on rollers with loose axles running in slotted holes in the hanging-straps. This forms a simple and convenient method of providing for the easy running of the doors. Each leaf weighs 30 cwt., yet when kept properly greased they can be easily moved by one hand.

Knuckle Station.

At the angle of the pier where the canted arm begins (Fig. 2) the space between the curve of the railway and the intersection of

the two lines of parapet was used for providing accommodation for a secondary station—the main station being at the root of the pier. At the Knuckle Station waiting- and refreshment-rooms, with the usual station-offices, are constructed with face-walls of Kentish rag and brick partitions. The roof is formed of steel troughing, which follows the line of the curved platform. The interiors of the rooms are lined with match-boarding, except the lavatory, which is lined with glazed bricks. The drainage from the offices is carried under the platform through the parapet and down the west face of the pier to low water in a chase cut in the granite and covered by cast-iron plates bolted on to the face of the wall. A grating was left at the top of the chase to prevent the plates from being burst off by the sudden compression of the air during rough weather.

Pier-Head, Lighthouse, and Fog-Horn House.

The pier ends in a roundhead 65 feet in diameter, formed 3 feet higher than the quay-surface (Figs. 2 and 6). In the centre of the head a lighthouse was built, in which a double flashing white light of the fourth order was established, with the focal plane 44 feet above high water of ordinary spring-tides. The illuminant is incandescent gas obtained from the town supply. The lighthouse-column is of concrete faced with granite, the external diameter being 11 feet 9 inches at the bottom, and 9 feet 6 inches at the top, while the internal shaft is 5 feet in diameter. Access to the lantern is gained by means of iron ladders. Attached to the lighthouse is a fog-horn house, which also contains a gasholder in which may be stored a supply of gas sufficient to maintain the light. By the use of this gasholder a uniform pressure of the required 2 inches of water at the burner is maintained, which it was found could not be depended on from the town supply.

The fog-horn installation consists of an electrically-driven air-compressor, which delivers into a receiver of 100 cubic feet capacity, where the air is stored at a pressure of 40 lbs. per square inch. Connected with the receiver is the sounding receiver to which the air passes through a reducing-valve. The fog-horn is of the Trinity House reed pattern, working at a pressure of 10 lbs. per square inch, and giving a blast of 7 seconds' duration every ½ minute. The period of the blast is regulated by a clock-work arrangement which works the valve of the fog-horn at the required instant.

Widening of the Old Pier on the West Side.

The new face which was built on the west side of the pier was composed of blocks similar to those of the extension. They were deposited by one of the travellers, of which one leg was shortened to run on the high-level staging, while the other was carried on the deck of the old pier, which was temporarily strengthened for the purpose. The foundations were prepared with the aid of the bell, as was done in the extension, but considerable difficulty was experienced owing to the unstable nature of the old foundations and to the amount of debris which had been washed out from the old pier. Scores of old bags of concrete and masses of concrete were met with, as well as several blocks which had fallen bodily out of the old structure. It was often difficult to decide whether some of these obstructions did not still form part of the old structure, and whether their removal would not lead to a considerable fall of the shaky wall. The greatest care had therefore to be exercised throughout the whole length. As the new blockwork was brought up, it was backed with concrete-in-mass and bonded in with the old work as much as possible.

Apron.

In order to prevent scour and undermining of the foundations by sea-action, an apron of concrete, 13 feet wide, was laid along the whole length of the west side of the pier, and continued round the outer end of the pier to the outer boat-steps (Figs. 2 and 5). It was noticed during under-water examinations of the old pier that considerable scouring action resulted from the paddle-steamers always occupying one berth, and aprons were therefore provided at each berth on the east side, over the lengths likely to be affected (Fig. 2).

On the pier-extension the apron was laid in blocks from the staging. The blocks in every case were laid on the top of the greensand, and were not sunk into it, as were the foundation-blocks. Along the widening on the west face of the old pier the staging was not wide enough to permit of blocks being laid, and the following method was adopted. The cover over the greensand, which consisted of about 4 feet of sand and shingle, was removed by a grab worked from a portable crane, running on the edge of the blockwork.

It was found that sand and shingle accumulated very quickly in the dredged area whenever there was the least swell, and a 15-foot

square open frame, formed of light iron plates 3 feet deep was brought into use with good results. The material was removed from the inside by the grab, the divers clearing out the corners. Concrete-in-mass was deposited inside the box, which was raised by the crane as the depth of the concrete increased, and finally removed when a depth of 3 feet 6 inches over the top of the greensand had been attained.

Strengthening at Root of Pier.

The work of strengthening and protecting the root of the old pier presented problems of considerable difficulty, for the following reasons. One result of the previous extensions of the pier and of the efforts made to cope with the accumulation of shingle was that between the pier and the old stone groyne there was a re-entering angle, facing the worst exposure (Fig. 1). This caused an abnormal wave-stroke against the old buttresses forming the root of the pier. The amount of cover over the greensand and the necessary proximity of the new work to the old made it impossible for an open trench to be dredged without dragging down the whole of the old buttresses.

The ground surface over the area of the new works was covered with debris from the old pier in the form of rocks up to 4 tons, with old rails innumerable and of all shapes.

Cylinders.—It was decided to obtain a secure foundation by means of cylinders sunk into the greensand and to carry on the top of these a blockwork wall, backed with chalk filling, and thus to cut off completely the action of the sea on the dilapidated old buttresses The solid wall so built would by itself have caused a heavier sea to mount over the station than did the old buttresses, and a wave-breaker of pell-mell blocks of 20 tons weight, placed in the angle between the new wall and the groyne formed therefore an essential part of the design (Fig. 2).

The cylinders (Fig. 7) were arranged in two rows; those in the front row, and several in the back row, where obstructions were certain to be met with, were of steel, 11 feet in diameter and built up in rings 5 feet deep to a height of 25 feet. A temporary internal cylinder, 7 feet in diameter, attached to the top of the bottom or cutting-edge section, and 44 feet long, formed a shaft which extended to 7 feet above high water when the cylinder was home, for working the cylinders under compressed air. The annular space between the permanent and the temporary cylinder formed a convenient space for kentledge.

In carrying out the work, the staging was first erected and the

diving-bells were set at work clearing the area of some of the debris already mentioned. They were not, of course, allowed to excavate to any great depth or they would have defeated the very end for which the cylinders were devised. The old concrete apron had also to be broken up and removed before the first cylinder could be pitched.

When a site was ready to receive a cylinder, the shoe, which had already had the annular ring filled with concrete, was placed on a bench left on the nearest blockwork at low water. The upper rings were bolted on, as also were the first three sections of the temporary cylinder. The whole was then lifted by the 30-ton traveller and placed in position (the annular ring being flooded at once to overcome the buoyancy), and secured with wire ropes and timber guides to prevent movement in case of rough weather springing up. The annular ring was then filled with shingle for kentledge, the upper sections of the temporary cylinder and the air-lock were fixed, and sinking commenced.

It was frequently found that a rock which could not be removed by the bell projected into the cylinder from one side and caused considerable cant when the cylinder was first pitched. In such a case the bed was levelled by building up sand-bags, until the unsupported part of the cutting edge obtained a bearing. These were removed gradually as the rock was cut through. The obstructions met with caused the work to be very slow at first. Old rails formed the worst obstruction and gave considerable trouble, by skidding the cylinder out of position, before they could be removed. In a few instances they could be drawn out inside the cylinder, and in such cases they were hung up inside out of the way, as they could not pass through the air-lock; but in most instances they had to be cut. When once the cylinder had passed the stratum of debris, sinking proceeded much more rapidly.

Fortunately no rough seas occurred during the early stages of sinking any one of the cylinders, or the risk of the cylinder being displaced and damaged would have been very great. As a matter of fact, one day of rough weather materially assisted in sinking a cylinder which was well entered in the sand. Each wave-stroke shook the cylinder and caused it to settle considerably faster than the usual rate of progress. The peculiar result of the rapid expansion of compressed air was very noticeable in the cylinders during moderately rough weather. When a wave outside receded suddenly the pressure was immediately reduced and the surplus air rushed out round the cutting edge with a loud roar. Simultaneously with the sudden expansion of the air there was an instantaneous lowering of the temperature, with a consequent condensation of the moisture

in the atmosphere, so that in an instant a thick fog was formed which gradually cleared away as the pressure increased again.

When a cylinder had reached within 6 inches of its finished level further excavation was stopped and the cylinder was pressed home by loading it on the top with 20-ton blocks. A load of 180 to 250 tons was necessary to effect this. The cylinder was then flooded and the shoe sealed with 4-to-1 concrete in mass, deposited through the water. As the temporary cylinder projected above high water, a hole was provided below the level of low water, through which the tide could ebb and flow into the cylinder. This prevented the water from rising and falling through the green concrete in the seal.

The temporary cylinder was then drawn up by the traveller, the bolts connecting it to the permanent shoe having been removed just before the men finally left the cylinder. The shingle fell down and filled the permanent cylinder, whence it was easily removed by a grab. The cylinder was then filled with 6-to-1 concrete deposited in the water.

A length of 100 feet behind the front row of cylinders appeared to be fairly clear of obstructions, so that it appeared practicable to economize by sinking concrete cylinders, and eleven of these were prepared. The concrete cylinders were, like the steel cylinders, 11 feet in diameter. Their internal diameter was 7 feet, leaving a ring of concrete 2 feet thick. The cylinders were built in rings 5 feet deep, which gave a weight of 18 tons. The cutting edge was of cast iron 3 feet deep, which was topped with 2 feet of concrete formed to receive the upper rings. The upper rings were formed with projections 6 inches deep both top and bottom, so that they fitted together like a clutch in gear. The rings were made on the block floor and were lifted by a special tripod, which, when lifting, expanded and pushed the feet into recesses inside the cylinder provided with iron plates to prevent the concrete from being crushed. The rings were run out to the pier on specially-prepared bogies and placed in position by the travellers on the staging. These concrete cylinders proved to be very awkward to deal with and no real economy was effected. The main difficulty experienced was owing to the impossibility of controlling them properly in passing through ground which was not homogeneous.

The sinking of the cylinders was carried on in the usual way by grabbing out the interior and adding extra rings on top as required. All went well through favourable ground, but as soon as the least obstruction was met with the cylinder began to cant over. Efforts to keep it upright by strutting or bearing on guide-piles had no more effect than to open the joints between the rings

on the lower side, owing to the upper rings being supported stiffly on that side, while the lower rings, being already under water and in ground incapable of supporting them so strongly, continued to fall away from the vertical. Of course, as soon as the cylinder began to cant over, grabbing was stopped, and a diver was sent down to clear the obstruction. This was frequently a tedious business, and, owing to the great weight of the cylinder, movement usually continued on the soft or unobstructed side. One or two rings were sometimes removed in order to reduce this action and to prevent the cylinder, when canted away at the top from the next one adjacent, from creeping up to that cylinder at the shoe. When sinking was resumed, a stiff prop was inserted against the bevel of the shoe. In spite of all precautions one cylinder got hung up before it had quite reached the full depth, and as it could not be moved it was filled with concrete. The lower greensand into which the cylinders were sunk proved to be quite impenetrable by grabbing, so that when the grabs discontinued to act with advantage they were removed and divers were sent down the well to remove the remainder of the material. The progress of sinking through the greensand by divers was also found very slow, and a jet of compressed air was tried as an experiment and acted fairly well. The air-compressors on the staging used for the diving-bells and steel cylinders were brought into use. The pressure was pumped up to 80 lbs. per square inch in the receivers. Connected with the receiver was a 3-inch pipe leading to a convenient point on the staging from which a flexible hose 3 inches in diameter, as used for the bells, was led down to the diver at the cutting edge. The hose was fitted with a ½-inch nozzle. When the air was turned full on the water in the well was at once seen to become discoloured with greensand, and as the cutting edge was gradually cleared by the diver further sinking took place. Two divers were necessary to control the nozzle, and they were greatly handicapped by not being able to see what they were doing. Concrete cylinders in rings may no doubt be used with great advantage when the circumstances are favourable, but in this instance no economy was effected.

The cylinders were filled with concrete after the sinking was finished, and the spaces left between the cylinders were cleaned out as deep as possible, and also filled with concrete. The wall was then built on the tops of the cylinders by setting blocks in the usual way. The chalk filling behind the wall was brought up as the wall rose, and on the outside the wave-breaker of 20-ton blocks was also brought up.

Wave-Breaker.—The blocks forming the wave-breaker were

deposited from a staging which was here two bays in width as on the pier-extension. The blocks were laid as irregularly as possible in order to obtain the maximum interstitial space. The dolphins of the centre of the staging were necessarily surrounded by blocks, and as the piles were liable to be broken by their weight a pier was formed by boxing in the piles and filling the space with concrete. The blocks in the wave-breaker were all set by nippers, which, if they did not release themselves when the block was landed, could be opened by a trigger-line from the staging.

The number of 20-ton blocks deposited in the wave-breaker was 461. The effect of these blocks during a gale is very noticeable. A large wave which, against the solid walls in the re-entering angle, would have mounted over the top of the parapet, with serious results to the station lying immediately behind, is dashed into spray on meeting the wave-breaker, while the force of the stroke is spent in the interstices between the blocks. A great deal of light spray is thrown at times over the pier, but the weight of water is immaterial. The whole wave is practically sucked up by the wave-breaker and there is no recoil. It is interesting to note that this system of protecting structures against sea-action is of very ancient origin, for it is on record that Alexander the Great, when besieging Tyre in the year 332 B.C., was unable to bring his floating siege engines close enough to the walls to be effective until he had, with infinite labour, cleared away, by dragging down into deep water, the huge rocks which the Tyrians had placed round the foot of their walls to break the heavy seas.

Cylinder-Wall.—The cylinder-wall was carried up to the old stone groyne, into which it was bonded, the chalk filling was then brought up to formation-level and prepared to receive the buildings of the main pier-station, which was erected at this point.

DREDGING.

The preliminary survey of Folkestone Harbour undertaken before the works were commenced disclosed the fact that on the east side of, and about 200 feet from, the end of the old pier the water shoaled to a depth of 11 feet and less at low water of ordinary spring-tides. This shoal patch had not hitherto proved inconvenient to steamers making the pier, inasmuch as their line of approach was directly up the channel of deep water along the site of their berths.

As the pier-extension with its canted arm was nearing completion the line of approach was necessarily deflected more to the east, and brought the vessels over the shoal water. This rendered dredging

compulsory, and in 1901 about 60,000 tons were removed. In the period of 5 years which has elapsed since these dredging-operations, the area so deepened has gradually returned to its normal condition, rendering dredging again necessary.

The loss of depth referred to is greatly accelerated by the outfalls of the sewers, serving the whole of Folkestone, debouching directly into the sheltered area, where there is an eddy of the tidal currents producing an area of almost slack water which facilitates deposition of all the material brought down. Moreover, the detritus from the cliffs immediately to the east of the old harbour is considerable. The cliff is composed of upper greensand beds, gault clay and lower greensand beds, all of which suffer great loss owing to the action of the sea on the unprotected base. The material from the wastage of the cliff also circulates in the sheltered area and contributes to the deposit on the point referred to.

These causes will have the effect of restoring the sea-bottom to its normal depth, entailing dredging operations at intervals of about 4 years.

Foreshore-Protection.

Immediately westward of the stone groyne the foreshore was composed of shingle, which during moderate west winds accumulated against the groynes, but during east winds or rough weather disappeared to such an extent as to endanger seriously the main line to the pier. The loss of shingle which took place at this point was continuous during the whole of the time that the works were in progress, and the following reasons may be assigned for this. First, the travel of the shingle towards the pier from the west was being checked by the extension of groyning at Sandgate and along the foreshore to Folkestone. Secondly, the very questionable practice of removing shingle from the beach for the purpose of road-making and building in the town was largely resorted to, and large quantities of shingle, which in the normal course of events would have travelled on to the stone groyne, were removed in this way. Thirdly, the pier itself was responsible for a certain amount of the loss which occurred, owing to the waves which recoiled from the solid wall interfering with the normal waves from the west and effecting a flattening of the slope close to the stone groyne.

During a gale in December, 1902, considerable damage was done to the foreshore between the stone groyne and the west end of the work-yard. The main line was blocked for a short time, some of the signals were upset, and the signal-cabin was threatened with

destruction. A timber breastwork which had been constructed for the temporary protection of the block-yard was utterly demolished. It was, of course, very important that the main-line connection with the pier should be secured against all possibility of being broken, and foreshore-protection works of a permanent character were put in hand.

The old main line was carried by sleepers over double-timber way-beams on a timber trestle, which when erected about 50 years previously stood up 10 or 15 feet above the shingle. The shingle had gradually accumulated until the way-beams were buried and supported by it. The removal of the shingle left the whole load to be borne by the old way-beams, a function which after 50 years in damp ground they were ill capable of fulfilling. It was decided in arranging the new work that there should be no bridge-work of any kind, but that the road should be carried direct on the shingle, which should be guarded against removal by protecting barriers of rockwork inside piled frames of old rails (Fig. 2, Plate 1). Two of these barriers were formed, one at the level of high water of ordinary spring-tides, and the other at rail-level, with a face sloping seaward. Rail-piles were driven 12 feet to 20 feet long, 2 feet apart between centres, in two rows. Between these rows, which were 12 feet apart, rough rock, in pieces weighing 2 to 3 tons, was stacked. The piles were braced together with old rails, and rail-ties were fixed across the top. These barriers, while breaking up the sea by their rough surface, do not create a rebound, and act most satisfactorily in preventing undermining. It is possible, and even probable, that in exceptionally heavy gales some shingle may be thrown across the lines, but the possibility of any undermining is very remote. Besides the rock barriers a timber breastwork 325 feet long was constructed for the protection of the entrance to the yard. This was formed by driving counter-piles 12 inches by 12 inches, 5 feet apart between centres; and behind each alternate counter-pile, and 14 feet back from it, was driven a stay-pile, from which a 12-inch by 12-inch strut and a tie-rod 1½ inch in diameter were carried to the face. The face of the breastwork was covered with 4-inch sheeting, fixed horizontally to a depth of 7 feet below the point where a storm slope of 1 in 9 from low-water mark cut the sheeting.

The breastwork was backed with chalk, which was covered with a tar skin. In order to encourage the growth of the beach the existing groynes were put in repair and lengthened to 40 feet beyond low water. A new groyne was also constructed on the

same pattern as the old ones, namely, double piles of old rails, with 3-inch planking between, carried down the beach according to the average slope, and supported by rail struts on both sides. The lengthening of the old groynes and the construction of the new one have had a very good effect, and the appearance of the beach at this point is now perfectly satisfactory.

Renewal of the East Face of the Old Pier.

The last important work to be put in hand was the renewal of the east face of the old pier, which was in a very dilapidated condition.

The east face, being the one sheltered from the prevailing west wind, is the principal working-face, and it was considered desirable not to make a break in the line of the pier, but to keep the same line throughout if at all practicable. A careful under-water examination, however, showed that this was quite impossible. The rail-panelling, already referred to, was in many bays fastened to the front of the double piles, and to have removed this would have let down large portions of the pier. In addition to this, some of the old sloping blocks projected 2 feet beyond the face-line. As it was necessary to enclose the face completely by sheeting, it was decided to adopt a new line 5 feet in front of the old, and to drive continuous greenheart sheeting from end to end of the working-face.

The character of the work is shown in Figs. 4 and 6. The main piles are 15 inches by 15 inches, and the sheet-piles and walings 12 inches by 12 inches. In fixing walings to gauge-piles for a long length of sheeting the following point is worth noting. In some grounds the driving of continuous sheeting compresses the ground to such an extent that the piles first driven, i.e., the gauge-piles, are lifted to a level, it may be 4 inches, above their former position. In such a case it is important not to fix the top waling (upon which probably the level of the decking will depend) to any one gauge-pile, until the whole of the sheet-piling has been completed in the bay on each side of that pile. It is important, moreover, to allow for this lifting when fixing the lower walings. The level of a waling on a bay of finished sheeting should not be continued forward, but the next waling should be lowered at its outer end by such an amount as experience shows to be necessary to give a true level line after the sheet-piles are driven. The difficulty might be overcome by fixing the walings temporarily

only in the first instance, but this procedure entails the boring of extra bolt-holes and so weakening the main piles.

Before being driven the greenheart piles were riveted with a $\frac{7}{8}$-inch rivet, with 2-inch washers let in under flush every 10 feet to prevent splitting during driving. Hoops were also fixed on the piles while they were being driven, and although the driving proved extremely hard, so much so that a 30-cwt. monkey falling 4 feet drove the pile only $\frac{1}{100}$ inch towards the end, yet only two of the piles had to be removed because of splitting. The piling was tied back by 5-inch by $1\frac{1}{8}$-inch straps passing round the main piles immediately below the level of the lower landing, and connected to the old $2\frac{1}{2}$-inch tie-rods referred to before as having been inserted to tie the two faces of the old pier together. The space between the sheet piling and the face of the old pier was filled with chalk, over which the lower landing was carried on greenheart transoms covered by 4-inch slabs of greenheart and 12-inch concrete. The whole of the lower landing was paved with 2-inch chequered blue tiles, finishing in front against cast-iron nosings. Galvanized-iron guard-chains were fixed between the piles with a standard in each bay. The standards fitted into sockets in the nosings so as to be easily removed for gangways to be run out to the steamers.

A new face-wall at the back of the lower landing was built of granite blocks to conform with the rest of the work. The deck of the pier was constructed of steel box girders, of which the web plates formed a jaw in front enclosing the main piles, while the inner end was provided with an anchor-bolt and 20-inch square cast-iron washer-plate sunk into the concrete.

The way-beams were also of steel box girders, to which the rails, which were flat-bottomed, were secured by clips fastened to the girder by tap-screws. The decking was of 4-inch pitch-pine carried on tee-bar transoms between the way-beams.

Equipment, etc.

Permanent Way.—The permanent way was laid as a bull-headed-rail ($91\frac{1}{4}$ lbs. per yard), double-checked, sleeper road throughout the pier except over the lower landings, where flat-bottomed rails were used. Ballast 2 inches deep was laid under the sleepers on the top of the concrete for convenience of packing to adjust the level; and after the road was laid, concrete was deposited between the sleepers, and the surface was finished off with 7-inch granite pitching, which cambered 1 inch in the middle of each road for drainage;

while the surface as a whole sloped to the lee side of the pier for the same purpose. The channel between the running- and the check-rail was filled with asphalt to prevent water from soaking down to the sleepers, while at frequent intervals there were outlets from those channels connected with cross drains discharging just under coping-level on the east side. The check rail, besides performing its usual function, acts conveniently as a curb for the granite pitching, but it gave considerable trouble in the arrangement of the locking-bars and other signalling contrivances.

Signalling.—The system of signalling adopted is that of Sykes Electric Mechanical Company. The signalling is worked from a cabin erected on the pier.

Baggage-Cranes.—The system of dealing with the baggage of passengers to and from the Continent has been for many years on the Folkestone route as follows. The baggage at the termini is loaded into removable boxes, of which two are carried on a truck specially prepared to receive them. On the arrival of the boat-train at the pier, the trucks are run under a portable crane, which lifts the boxes and deposits them on the steam packet-boat. Owing to the increase in size of the paddle-steamers, it was becoming more and more difficult for the short-jibbed cranes in use to do the work, and it was impossible for them to deal with vessels lying at either of the new landings on the west side of the pier because of the intervening parapet. Two new cranes of a more suitable type were therefore obtained. They are portable steam-cranes capable of dealing quickly with a 4-ton load at a radius of 40 feet. In order that they might be in a position to deal with vessels in any position on either side of the pier they are mounted on steel-framed travelling gantries, spanning the inner and outer arms of the pier respectively. The cranes are carried by web girders, set 20 feet clear of the deck of the pier, supported by one long side frame running on coping-level at the east side of the pier, and by one short leg running on a rail sunk into the granite capping on the parapet. There is thus no obstruction to the traffic on the pier, while the cranes, besides being able to move to any desired position, obtain the benefit of a 10-feet-square wheel-base, which is much larger than could have been provided for them on the low level.

The girders of the travelling gantry project over the parapet on the west side so as to permit of the cranes plumbing the largest paddle-steamer in the railway-companies' fleet when lying at a western landing.

Electric Lighting.—The pier is lighted throughout by electricity.

The light for the deck is provided by arc-lamps suspended from lattice masts, while the platforms and lower landings are lighted by glow-lamps. Those on the lower landings are enclosed in strong lanterns of cast-iron frames with $\frac{3}{8}$-inch plate-glass fronts and sides. The electric signals are also lighted by electric glow-lamps in lanterns specially made so as to provide for substituting rapidly an oil-lamp in the event of any failure in the electric light.

Gas- and Water-Supply.—Gas- and water-mains are laid along the pier between the roads up to the lighthouse. The water-main is provided with fire-hydrants at frequent intervals, and with connections for watering the steamers at every berth.

The work was carried out from the designs and under the direction of Messrs. Coode, Son and Matthews, to whom the Author, who acted throughout as Resident Engineer, desires to express his thanks for assistance in the preparation of this Paper, and for the loan of plans.

Mr. William Rigby was the Contractor, and his Agent was Mr. James Grice, to whom also the Author's thanks are due.

The Paper is accompanied by a chart and eight drawings, from which Plate 1 and the Figures in the text have been prepared.

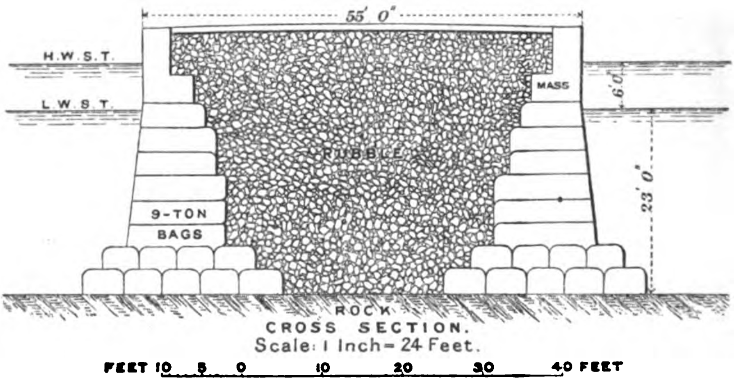
Discussion.

The President. The PRESIDENT moved a vote of thanks to the Author for his clear and interesting communication.

The Author. The AUTHOR mentioned that when visiting Folkestone a few days ago he had been pleased to observe that the works were in a very satisfactory condition, especially the foreshore at the west end, where the shingle had accumulated to an extent which he had not anticipated. The timber breastwork shown in Fig. 2, Plate 1, was topped with shingle, and there was a bank in front of it quite 20 feet wide. The Author then exhibited some lantern-slides illustrating the work.

Mr. Dyce Cay. Mr. W. DYCE CAY thought it might interest the meeting to hear a short description of the steamboat-pier (*Fig. 10*) which he constructed at Lerwick, in the Shetland Islands, in 1882.[1] In such a situation, of course, economy had to be kept always in view, as there was no Continental traffic to pay for luxuries. Still,

Fig. 10.



as the work had answered all its purposes for upwards of 20 years and continued to do so, it did not appear that more could have been spent upon it with advantage. The site was in Bressay Sound, and the pier ran out from the centre of the town about

[1] W. D. Cay, "The Construction of Marine Works with Concrete Bags, and the Plant used for their Deposit" Transactions of the Royal Scottish Society of Arts, vol. xii (1891), p. 121.

Mr. Dyce Cay.

300 feet. The rock bottom shelved very rapidly, so that at the end of the pier there was a depth of 29 feet at low water, which was not needed, as the steamers only drew 12 to 15 feet. However, it had advantages in other respects. The bottom was covered with 12 to 15 feet of sandy mud, which had to be dredged before the rock bottom was reached. The dredging was done with a Priestman grab worked upon a barge, 50 feet long, 24 feet wide, and 6 feet deep. The barge also carried a steam-crane, and it not only did the dredging but also laid all the bags. He mentioned that to show the great economy of the plant as compared with the vast staging used on some breakwaters. With the dredged material an esplanade was formed along the front of the town, which was a great advantage, because previously the houses stood quite close to the water. As soon as the rock was bare, the foundation was laid with 9-ton concrete bags, kept wider at the base of the wall and laid longitudinally, as stretchers, on the two lower courses. Above that the bags were all laid as headers. The skip which laid them measured 12 feet by 3 feet and was 4 feet deep. The bag was put into the skip, filled with concrete and sewn up, and then guided into position by divers; and as soon as it was in position a trigger released the bottom of the box and the bag was deposited in its place. Work was carried up thus to low water, above which concrete in mass was used. The pier was not solid like the Folkestone pier but consisted of two side walls with a hearting of rubble. The top was a concrete floor on which was placed a timber shed. The narrowness of Folkestone pier surprised him, the outer reach of the pier having less than 40 feet of deckway between the inside of the parapet and the edge of the coping. He had been on the railway-platform as a passenger and had found it was only 7 or 8 feet wide. Perhaps the Author could give some explanation of this narrowness. It might have been done for the sake of economy; if so, there was some reason for looking into the cheap system he described. The sides of the Lerwick pier were protected by timber fenders and sponson-fenders, as some of the steamers in the trade were paddle-boats. The cement came from the Thames and the sand and gravel from the sea-shores outside the Sound. There was nothing close at hand, so that the materials were a little more expensive. It could easily be seen that such a work was comparatively cheap. There was no sea-staging and no large cranes, and the plant needed was nothing compared with the vast plant required for setting the blocks and sinking the cylinders, and for diving-bell work, etc., in building the Folkestone pier. The little sea that was met

Mr. Dyce Cay. with in severe storms on the toe of the esplanade at Lerwick was protected by bags which were laid by the same plant. The 20-ton concrete blocks at Folkestone could only be stable in a place where there was not much sea. Anywhere on the east coast, such as Aberdeen, or the Tyne, or Wick, the 20-ton blocks would be reduced to gravel in the course of a couple of years, but they probably did quite well at Folkestone where there was not much sea. The French had found that pell-mell blocks were much broken up by heavy seas. The remedy he had suggested at the Milan International Navigation Congress, 1905, was that instead of placing vertical walls above the riprap, it would be better to increase the height and width of the riprap bank and protect it with big blocks, bags of concrete, or concrete cylinders with rounded ends, all placed as a revetment on the slope. The plant he had referred to had also served for constructing the wharves along the esplanade at Lerwick. The contractor's price for the concrete in bags was 20*s.* per cubic yard, and for concrete in mass was about 18*s.* per cubic yard; to each of these figures 2*s.* 6*d.* had to be added for plant and temporary works. With regard to the experiments made by the Author, by depositing dry concrete in bags and allowing the sea-water to percolate the mass, this system had been mentioned to Mr. Cay 6 years ago by Mr. R. G. Nicol, M. Inst. C.E., the harbour-engineer at Aberdeen, who had tried it and had produced very hard blocks. There was no difficulty as long as the concrete was mixed in the usual proportions 4, 5, 6 or 7 to 1. The Author seemed to think that the water might not get beyond the crust, but Mr. Cay thought it would always penetrate the mass under those conditions, owing to the porosity of the concrete. The only disadvantage was that sea-water was not so good as fresh water for mixing with cement and might ultimately cause weakness in the concrete. He thought the Author need not be alarmed about neat-cement experiments showing gradually less tensile strength in the course of years, because that had been found to be the case, certainly in the French experiments. In fact he came to the conclusion many years ago that it was better to use cement with a little sand, 1 to 1 or $\frac{3}{4}$ to 1, as it proved in course of time to be a better article for sea-work than neat cement.

Mr. Hurtzig. Mr. A. C. Hurtzig thought the Paper was one of exceptional interest and was a record of difficulties so successfully overcome as to be beyond criticism. He would have preferred to see the inner face of the pier vertical instead of battered 1 in 12. The tendency in the development of the shape of ships was such that the battering of faces of dock-walls, piers, and quays was becoming a greater

disadvantage every year, and he thought it would have been better if the face had been vertical. One of the most interesting parts of the Paper was that which referred to the aeration of the cement used for the concrete. It was remarkable how differently engineers looked upon those matters. He knew of works of excellent character which had been carried out with cement used as delivered on the site, without any aeration whatever; but he inclined to the belief that aeration up to a point was the proper course to be adopted in carrying out such works. He was rather sceptical as to a contractor accepting the dictum of the Author that cement could be turned over six times without cost. If it were so, he thought the work must suffer by having less cement in it; it might be cement of a better quality, but there was less of it. Cement was specified to be of a certain specific gravity, and it was accepted if it was of that specific gravity; but after aeration it was used not at that specific gravity but at a lower one. He was very glad to see from the Paper that so experienced an engineer as the President continued to use the old-fashioned Messent mixer. Many new mixers had been introduced, but in his opinion—based upon long experience of concrete work—none produced better concrete than the Messent mixer. The pile-driving for the inner face seemed to have been somewhat laborious. As far as he could see, the piles did not carry any very great weight, and he thought the difficulty of driving might have been reduced to some extent, and that the piles need not have been driven into the greensand. Mr. Hurtzig.

Mr. C. S. Meik asked whether the cement which had been used, and on which the tests had been made, was produced by the new rotary process or the old process. [The Author replied that it was produced by the old process.] That point required to be considered when looking at the diagrams. It was well known that pure cement was quick-setting, and it was only by the addition of impurities, such as the clinker from coal-fuel or by adulteration in some way, that it became slow-setting; that was why the "rotary" cement set so quickly when it was fresh from the works. It was so quick-setting that recently some cement he had had sent up to works on the west coast of Scotland set before it got into the work, and consequently could not be used. That particular lot of cement set in 2 to 3 minutes. It was kept, with a view to aerating it, for 8 months, and the time of initial set was then prolonged to 1½ hour, and the final set to 12 hours. That cement was not bulked, but was kept in bags and aerated in them. It was one of the peculiarities of cement made by the new process that if turned out into bulk it became more quick-setting, Mr. Meik.

Mr. Meik. while if kept in bags it became slow-setting: that was quite contrary to what engineers had been accustomed to with the old cement, which always had to be turned out into bulk in order to aerate it. With regard to the tests which the Author had made, Mr. Meik had had occasion recently at Swansea to compare cement made by the two processes and had found that cement made by the new process deteriorated in strength after it had been made into concrete and kept some time. Some 6-inch cubes of 4-to-1 concrete were made in order to compare the old with the new. They were tested at 3 months and at 6 months, and it was hoped to test them again at 12 months. The results of the tests were that the blocks of the rotary cement at 3 months stood 161 tons. The cement made by the kiln process, tested in blocks of the same dimensions and mixture, only stood 76 tons. But after testing similar blocks kept for a further period of 3 months, it was found that the strength of the rotary-cement concrete had fallen to 157 tons, while the strength of the kiln-cement had risen to 92 tons, showing a marked increase in the ordinary kiln-cement concrete and decrease in the rotary-cement concrete. As the cement used at Folkestone was kiln-made, he thought the Author was possibly right that gypsum was the cause of the reduction. At the same time, he did not see how it was possible to do without gypsum in rotary cement. It was not always feasible to keep cement for 8 or 9 months before using it. The only way of getting over the difficulty was to add gypsum. After Mr. Dyce Cay's statement that concrete blocks on the east coast would not last, Mr. Meik thought it was time he went to look at some of the works his firm had built there, to see if they were still in existence.

Sir William White. Sir WILLIAM WHITE, K.C.B., Past-President, had nothing to say about the technical merits of the structure or about the methods of mixing cement; those were subjects which, although they were of great interest and importance, lay outside his experience. But it so happened that during his Presidency of The Institution the great work at Folkestone was opened, and in his capacity as President he had the honour of being there and witnessing the ceremony which then took place. It was a ceremony of an interesting character, when many eminent Frenchmen came across and the French Ambassador was the principal personage. There was only one opinion among those present, an opinion which he thought all subsequent experience had confirmed, namely, that Sir William Matthews had designed a work which was admirably adapted for the purpose intended to be served. That was really the point of primary importance. It was possible for an engineer to build

Sir William White.

what would be a monumental work, as an example of his skill and daring, and yet not fulfil the fundamental condition of adapting the work to the necessary service. That could not be said of the Folkestone pier. He had been there many times since the opening day, and he had not noticed any congestion such as one of the speakers thought was likely to occur. The passenger-services on to the pier, the access of the steamers to the pier, and their departure, had all been evidently well thought out, and as he had said, the pier admirably served its intended purpose. It also had a capacity for meeting extension in the dimensions of ships: the outer berth appeared to be nearly 600 feet in length, and it would be a long while, he thought, before steamers employed in the Channel traffic would approach anything like that length. Mr. Hurtzig had made one point which it might be Sir William White had not quite followed. So far as his observation went, a batter in a pier was by no means always a disadvantage. He had seen cases where ships alongside vertical walls of piers in exposed positions had been the worse for the fact that the walls were vertical. No doubt the point had been very carefully considered; and as far as his observation at Folkestone went, he thought the batter of the pier was rather an advantage than otherwise. In work of that kind where sea-action had to be met, experience in many places had shown that the earlier engineers did not sufficiently appreciate the depth to which disturbance might be carried where there was a great fetch of the sea and where the waves to be dealt with were of great length, and therefore where the range of orbital motion of the particles in wave-water might be considerable. Everyone was familiar with illustrations of the fact that frequently sufficient provision had not been made. Having for many years been deeply interested in the subject of wave-motion, he had listened with the greatest interest to the statements made in the Paper as to the damping effect produced by the wave-breakers—mere piles of concrete blocks. One could understand how that must be when one learned the fact, but it would be interesting to know in what way the device of using huge blocks thrown down pell-mell, for making impossible the orbital motion which alone could maintain the wave-form, had come to be thought of. According to the Paper, the result seemed to be very remarkable, and when one knew that it happened it was also quite explicable. He had heard of cases where the same thing had been effected in other ways, where chain cables had been used to secure blocks, interlacing them and keeping them in fairly constant positions. It appeared to him—looking at the matter from the point of view of one who knew something about wave-motion

Sir William White. and its effect on ships, and whose business it was to know something about the theory of waves—that, as regarded maintenance of the structure, in its form, in its cross section, and in its protection, the designer of the Folkestone pier had exercised very great skill. He hoped he might not be thought to have gone beyond the province of a mere naval architect in making those remarks about fixed structures, but having been familiar with the work from its opening, he could only say it was one of which the President and those associated with him might very well be proud.

Mr. Inglis. Mr. JAMES C. INGLIS wished to add a few words to what Sir William White had just said about the batter of pier-walls. He quite endorsed his opinions on the matter, because he had been inclined to be a little new-fashioned himself on one work the Great Western Railway Company had just finished at Fishguard, a work in very similar circumstances to the Folkestone pier, and had made a wall which had a batter up to 18 inches above low water, and above that was vertical. As soon as the steamers began to use the quay-wall, £7,000 had to be spent to give it a batter. The reason was simple. While a vertical wall was quite correct in a floating dock, it was far from being correct in a quay exposed to a certain amount of sea. Vessels had fairly vertical sides now, and when steamers rolled against a vertical wall they damaged their upper works. He was bound to say he admired the economy of the Folkestone work, which was not like the Lerwick work, as the designer had not had quite a clean sheet to work on. The way in which the engineers had been able to face up the working side with the piles and get a new solid wall within such a little space showed, he thought, the highest economy. The work seemed expensive, but there was little of it, and what there was was good. With regard to the section of the pier, it seemed to him that 50 feet were ample for any wave-stroke that might take place on the outer portion, and he could not quite think that the whole of the story had been told with regard to the Lerwick pier, where the walls at the top seemed to be about 3 feet 6 inches thick. He did not think there could be much fetch there, because clearly work of that kind would not stand in a place like Folkestone.

Mr. Dyce Cay. Mr. DYCE CAY asked permission to correct a misapprehension with regard to the 20-ton blocks. He had referred to pell-mell blocks, not to blocks built solidly in the pier itself.

Mr. Beard. Mr. E. T. BEARD asked for an explanation as to the size, form, and position of the grout-cavities. With regard to the testing of cement, it occurred to him that if the cement was not used until after it had been turned over five times and stored for 1 month, there might have been a considerable saving of time and labour

by the use of the boiling-test after the cement had been seasoned instead of before. The diagram showing the effect of the addition of gypsum indicated that the strength of the neat cement diminished by 50 per cent. in 3 years; he wondered whether, had testing been carried out for another 3 years, it would have fallen 100 per cent. Mr. Beard.

Sir WHATELY ELIOT observed that Folkestone harbour seemed to be a good example of a harbour constructed under the greatest possible difficulties, and of great perseverance extending over several hundred years. A harbour existed at Folkestone 300 or 400 years ago, but it was constantly being choked by shingle; in fact the shingle travelling along the coast prevented the construction of any good harbour. Among the records of the Corporation of Folkestone there was one dated, he believed, 1635, which referred to the existence of a standing order that when the sound of a horn was heard in Folkestone all the householders had to turn out with shovels and other suitable tools, go down to the harbour, and clear away the shingle. There was no record of their getting paid for their labour, but it appeared that if they did not go they were fined. That state of things seemed to have gone on for many years, but towards the end of the eighteenth century the harbour was again completely choked. Whether the householders of Folkestone had by then given up the practice of clearing out their harbour he did not know. At the beginning of the nineteenth century, when the design of the present harbour was first prepared by Telford, they set to work at once with the pier which formed the wall on the west side of the basin. The work was persevered with until it reached nearly to the end of the wall forming the quay on the south side of the basin, but when that point was reached the constructors became thoroughly disheartened, the funds came to an end, the shingle was still encroaching and constantly filling up the approach to the harbour, and the work was accordingly abandoned. The next thing he could gather about the harbour was that in 1842 the South-Eastern Railway Company purchased it as it then stood, and immediately set to work to clear it out. It was completely silted up, but they cleared the shingle away from the entrance and continued the wall forming the south quay to the harbour-entrance, and then built the pier or horn stretching southward. That, as far as he could make out, was really the first real stoppage of the shingle; it prevented the shingle from getting into the harbour-entrance, and allowed the harbour to be cleared out. In 1862 a low-water landing-place was made, described in the Paper as being formed chiefly of iron rails and a mound of rubble. It did good service, forming a landing-place for the steamers, and stood there until Sir Whately Eliot.

Sir Whately Eliot. 1882, when it was widened, lengthened, and raised. The work was very substantially built, was faced each side with concrete blocks weighing 15 to 16 tons, and the concrete seemed to have been of good quality; but 15 years later the wall was being destroyed by the sea. In the Paper this was ascribed to three causes, namely, insufficient foundation, the irregular way in which the blocks appeared to have been set, and the bad quality of the concrete. He was inclined to think that the insufficiency of the foundation was the cause of the mischief. However well the blocks were made, if the foundation was not good, the sea was sure to force them out of place in course of time. The extension described in the Paper appeared to have been carried out by an excellent method. When he saw the magnificent staging used on such works and compared it with what he had been accustomed to in his younger days, he began to wonder how it had been possible to get any works to stand at all in those earlier days. The fact that the Folkestone work had been carried out under heavy seas, and that the sea had never done any damage to the staging, was a proof that the latter had answered its purpose. With regard to the strengthening at the root of the pier, anyone who knew anything about sinking cylinders would agree that there could not be a more unfavourable piece of ground for such work than that described in the Paper, consisting of masses of rock up to 4 tons in weight and pieces of rail of all shapes and sizes. After his own experience he was not surprised to hear the Author's opinion that in ground of this nature there was no economy in using concrete cylinders instead of iron cylinders. The wave-screen seemed to be a very good arrangement for reducing the percussion of the heavy seas in the angle. He had noticed on several of the old piers in the North of Scotland that a screen or a long slope was invariably put at the back of the piers, the idea being that the sea should not get a fair blow at the piers. Those who had seen the method of constructing a good many of the piers would not wonder at this precaution being taken. The Paper described an excellent structure, which in fact bore a strong family likeness to the Dover work and the work at the Tyne, and was in every way as substantial-looking and as satisfying to the eye. It was a fitting finish to the old harbour of Folkestone, and he supposed it was the finishing touch—at all events for several years to come.

Sir John Wolfe Barry. Sir JOHN WOLFE BARRY, K.C.B., Past-President, having seen the work at Folkestone in many stages of progress, and having had some knowledge of the old pier, wished to say a few words on the subject of the Paper, which he thought was a very interesting one

Sir John Wolfe Barry.

from many points of view. The old pier was certainly a very extraordinary structure. How it held together he could scarcely understand. It was built in the roughest possible way, much of it by temporary expedients, with old rails and timber, and altogether was a forbidding-looking structure. He could not help thinking that Sir William Matthews, when told that he had to uphold it, must have thought he had before him a task such as few engineers had had to perform, and certainly such as he had never had to undertake before. This very difficult work had been carried out satisfactorily; in fact, the old pier was now like an unpleasant fly in a fine bit of amber. He thought engineers would recognize that a work of that nature called for great skill, ingenuity, and adaption of means to ends. The work had been far more difficult than one begun *de novo*, where the engineer could determine beforehand the method of construction to be employed. At Folkestone different parts of the work had had to be done in different ways, and the greatest possible care had had to be taken that no interruption of traffic should take place, or the old pier be damaged by the new works. With regard to the station-accommodation upon the pier, he certainly would like to have seen wider platforms and generally more accommodation for the traffic between England and the Continent. On the other side of the Channel larger ideas and views upon the matter seemed to be entertained, particularly in regard to traffic between Calais and Dover. For traffic of this nature platforms 15 feet wide seemed small, but he had not the least doubt that they were as large as the money available would allow to be built. Perhaps, however, those who had had the responsibility of prescribing them might have had floating in their minds an idea that, after all, the traffic might not be accommodated for many more years in that particular way, and that sooner or later—and he thought sooner rather than later—there would be train-ferries and a better way of transferring goods and passengers across the Channel. Other nations had grappled with circumstances of the kind by ferries, in a manner which in England did not seem to be completely realized. The expenditure at Folkestone must have been very large, and he could not help feeling that perhaps the money might have been better spent in improving the accommodation at Dover, and making a really good Continental station there; but that, of course, had nothing to do with the engineers. There were two English companies concerned which had different views, although they were to some extent united by a strange kind of tie. He was afraid it was only a temporary arrangement, and that each company looked somewhat askance at the development of the two ports. The London,

Sir John Wolfe Barry. Chatham and Dover Railway Company, he thought, had their eyes fixed upon Dover, while the South-Eastern Railway Company fixed their gaze upon Folkestone. It seemed a pity, because the traffic, although it was important, was not so very large in relation to the expenditure undertaken to accommodate it. Another point in connection with the work itself occurred to his mind as of considerable importance, namely, the effect that the further projection of the Folkestone pier might have upon the travel of shingle from west to east, and consequently upon the cliffs and country lying between Dover and Folkestone. He had had that important matter brought before him in former years, when the South-Eastern Railway was destroyed by landslips and the traffic wholly stopped for some months, and subsequently when he was asked, in conjunction with his nephew, Mr. Arthur Barry, to endeavour to devise steps to arrest further destruction of the cliffs and of land lying at the foot of the cliffs, called the Warren, between Folkestone and Dover, the movement of which was threatening further injury to the railway. It would be recollected that that railway was carried through a piece of land which was very subject to change—undercliff land which had slipped down from the higher levels and was more or less subject to movement towards the sea. The whole of the district along the coast between Dover and Folkestone was subject to gradual erosion owing to want of the shingle, which was the protection that Nature formerly afforded to the foreshore. He thought that there might be some considerable danger that the shingle would be arrested more or less—and, over a long series of years, perhaps more rather than less—by the projection of Folkestone pier. If so, he could not help thinking that there would be further dangers threatening the Warren, the Abbot's Cliff tunnel and the Shakespeare's Cliff tunnel. Those matters were of extreme importance. For good or bad the projection had been made. The effect of the old pier was, he thought, undoubted, and possibly the present extension might aggravate it. Engineers who had anything to do with work upon the south coast of England knew of the extraordinary natural phenomenon of the travel of shingle from west to east, which kept up the supply of shingle upon the shore at different parts of the Channel. For example, there was the immense deposit of shingle at the Chesil Bank, almost the whole of which came from the west. At Dungeness, again, there was another enormous body of shingle brought from the west. Farther to the east was Hythe, adjoining which in ancient days there was an old Roman port—the principal port of communication across the Channel—which port was now high and dry $1\frac{1}{2}$ to 2 miles,

or more, away from the sea, the intervening distance being entirely occupied by shingle which had travelled from the west. What effect the prolongation of the solid pier at Folkestone might have upon the portion of the shore and coast between Folkestone and Dover it was very difficult to say. It might be that the shingle would be so deflected by the pier that it would never touch either Dover or the intervening land at all. If so, he thought it would be found that, whatever efforts were made by the projection of small groynes into the sea to arrest the shingle, very little would be collected; and if very little was collected there would be further damage to the intervening coast. Those matters were perhaps of more importance to the whole body politic than the works at Folkestone themselves; but still Sir William Matthews had given consideration to the point, and perhaps he could say what he thought about it. Sir John Wolfe Barry could not help saying that he felt some anxiety on the subject. No one wished to stop the development of important places; but when it came to a question between Dover and Folkestone as alternative ports, it was almost a pity that so much money had been spent on Folkestone when the distance between Folkestone and Boulogne and Dover and Boulogne was practically negligible, especially with the steamers now crossing the Channel. He thought perhaps those considerations might possess interest for some who had studied the question in other parts of the English Channel. As he had said, it must be recognized that a very interesting work had been carried out in a highly satisfactory manner, and that it was a work presenting unusual difficulties, both in design and in execution. Sir John Wolfe Barry.

Mr. Bertram Blount observed that a good deal of the Paper, and a very important part of it, treated of the use of cement. The Author spoke of cement as it was at the date of the beginning of the work, about 1897. At that time Mr. Blount examined samples of this cement, and although he did not examine it completely, he analysed a large number of samples. On account of the interest of the Paper generally, and because it enabled a comparison to be made between what was done then and what was being done now, he had turned up a number of those old analyses, and had compared them with the present composition of cement, and he was surprised to find that the composition was substantially the same. By "composition" he meant ultimate composition, which was the only kind of composition that a chemist usually spoke of in the matter of cement. With regard to the proximate composition of the two classes of cement, the cement of 1897 and the cement of the present year, he was not able to speak: very probably they differed entirely, but it remained Mr. Blount.

Mr. Blount. a fact that the ultimate composition was exactly the same. With regard to certain points of detail under the head of "Materials," there was a description in the Paper of the method then in vogue of testing the soundness of the cement by boiling the briquettes, which at that time was regarded as too drastic a requirement. Nowadays, instead of being severe it was a customary proceeding, and he thought that fact afforded good proof of the advance that cement-makers had made. Whereas 10 years ago they objected to the treatment of their cement in this manner, now they not only accepted it but also adopted it. That appeared to him to show that the manufacturers had progressed, *pari passu*, with the requirements of the users. He also found that at that date, under the auspices of Sir William Matthews, the practice of turning was resorted to. Interesting suggestions, which almost approached a detailed statement, were given by the Author as to the advantage of that turning. Mr. Blount concurred with the statement that at that date the advantage was considerable, but he maintained that at the present day the advantage was negligible. He doubted whether any manufacturers who might be present would dissent if he said that at the present day they were competent to turn out a cement so sound that turning-processes involved unnecessary expense. Nevertheless, he sustained the opinion that at the date in question, 1897, the turning was very properly performed. He would like to ask if the Author could say what increase in bulk occurred to the cements which he examined, giving the analyses and saying why he considered any particular cement had increased in bulk more than another. It might add to the value of the Paper if such numerical data were available. As to the fineness of cement, it was an unfortunate fact that, other things being equal, the more finely a cement was ground, the more quick-setting it was; and although every user and every expert knew that the finer the cement was ground the sounder it was, yet there was the practical difficulty that finely-ground cement set more quickly, because its particles, being smaller, obtained more easily the water necessary to their hydration. To overcome that was a matter of extraordinary difficulty, and he really thought at the present time, when cement of unimpeachable quality was made, and when makers and users were both satisfied, that that particular difficulty was the crux of the whole matter. If it were possible to control the setting-time of cement without impairing its soundness and without altering its workability, so as to be sure that concrete made from it could be handled with ease within the very moderate time that modern concrete-mixing demanded, then he thought all cause for anxiety would be removed. The only

Mr. Blount. other point that occurred to him as of general importance related to the set of tests which purported to show that the addition of gypsum in moderate proportions—he was not speaking of the 10 per cent. that had been named—tended to decrease the strength of cement. He read the results of those tests in quite another way. It was true that neat cement, especially modern neat cement, finely ground, decreased in strength with age; neat tests generally did at a long date; but that, he believed he was perfectly safe in saying, bore no relation at all to the strength of the work. He had made many hundreds of tests in that way and he had found—and no doubt others had found too—that the strength of mortar, and still more of concrete, went on increasing. He thought the decrease of strength with the neat cement need not cause the least anxiety.

Mr. Carey. Mr. A. E. Carey remarked that the historical part of the Paper was particularly interesting to those who had carried out works in Channel ports, and who were familiar with the action of littoral drift, which was so strikingly illustrated about Folkestone. The Paper showed that when the arresting barrier at Folkestone had been from time to time gorged with shingle, and the pier was extended farther and farther into the sea, the same action of gorging took place from time to time, and the result always had been, and he believed was approaching again, that the pier itself became outflanked by the drift. It appeared also from the Paper that the now almost obsolete practice of artificial scour had been at one time tried at Folkestone and had failed, as it had failed elsewhere. The Author emphasized the curious fact—which Mr. Carey also had brought to the notice of The Institution—that within recent years there was an apparent reduction in the quantity of shingle travelling along the Channel coast. The explanation which he had ventured to offer some time ago was that not only were more groynes being built, which caught and retained the shingle in increasing quantities, but the actual source of the shingle showed some signs of exhaustion. There was no doubt that the immense quantities of shingle and sand which were indicated on the charts as lying on the bed of the sea at the eastern end of the Channel came from the vast deposits of gravels, principally tertiary, which at one time capped the chalk, and which had been washed down into the Channel when it came into existence. The action was a diluvial one. No doubt those gravels had drifted slowly towards the coast-line and then by intermittent sea-action they had been thrown up from time to time. The steady depletion of the reserves of the travelling medium of defence for the coast had, he thought, tended to bring about the present unsatisfactory state of affairs with regard to coast-erosion.

Mr. Carey. He had recently had occasion to make a careful survey of the coast-line at and near Langley Point, east of Eastbourne, and he had found that since the Ordnance Survey, which was corrected in 1899, the coast-line at that spot had receded a distance ranging from 70 to 100 feet. The practice of removing shingle for building-purposes, which had been universally condemned, still went on unchecked all round the coast. Moreover, the shingle, upon which the maintenance of threatened portions of the coast-line depended, was allowed to accumulate in places where it was often not only useless but actually detrimental. With regard to the pier itself, he had had an opportunity of going over the old pier at the time when it was in imminent danger of destruction. The repairs described in the Paper were then being carried on by using small sacks of concrete, and he understood at the time that the sacks were being supplemented with mass concrete. The extension and strengthening was certainly a highly successful piece of work. The Author did not give the cost of the various sections of the work, and Mr. Carey thought it would be of great interest if he would state the cost, per lineal foot, of the breakwater, disregarding its equipment as a landing-stage. The staging itself must have been very expensive, and if the Author would state the cost of the staging per lineal foot, irrespective of the plant employed for block-setting, it would be useful. Taking two works in close proximity to Folkestone, for the execution of which he had been personally responsible, in one case as Resident Engineer and in the other as Chief Engineer, he might point out that the Newhaven breakwater, complete in all respects, had cost something under £60 per lineal foot, and had been carried out without contractors. The other work was Hastings breakwater, the western arm of which had been carried out 1,100 feet into the sea: its height at the extremity, from the deck to the foundations was 52 feet. That breakwater was constructed 10 years ago. The contract-price for the work, including the parapet, which had not yet been constructed, was equal to £63 6*s.* per lineal foot. It was of a much lighter section than the Newhaven breakwater, which was based on sack-blocks of 100 tons weight, the superstructure throughout being in 8-to-1 mass concrete. Hastings breakwater was built in mass concrete throughout, the portion up to low-water mark (5¼-to-1 concrete) being deposited from skips. The superstructure was built in mass concrete in a similar manner to that of Newhaven breakwater. The resulting work in both cases had been an entirely homogeneous structure; there were no joints in any portion of either work. With regard to the Author's remarks at p. 62 about "creeping" of the blocks in the foundation-

course, the merits of the block system as compared with the mass-concrete system were, he thought, fairly arguable, under the conditions and limitations they presented, in the three contiguous works, Folkestone, Newhaven, and Hastings. The rate of progress was about the same in both cases. The sub-structure of sack blocks employed at Newhaven, and the mass concrete adopted at Hastings, had not been subject to any such movement or condition as that described in the Paper. The foundation at Newhaven was chalk; that at Hastings was, on the shoreward portion soft standstone, and on the outer portion angular gravel. The Author's remarks as to the treatment and testing of Portland cement were of considerable interest; he did not say, however, whether his briquettes were gauged with salt water or with fresh. Using sea-water, Mr. Carey had found that briquettes showed their maximum tensile stress at about 9 months, which period seemed to be corroborated by the Author's diagrams. The increased rapidity of setting which the Author noted with modern finely-ground cements was, as pointed out by Mr. Blount, due no doubt to the increased superficial area of the particles exposed to chemical action. The coarse particles of cement which resulted from the more imperfect grinding in vogue a few years ago were for practical purposes equivalent to an admixture of a corresponding amount of sand. About 40 per cent. of most of the cement manufactured 20 years ago possessed really no cementitious properties; it was merely equivalent to dilution by a neutral body. The fact was that, using cement ground to the standard fineness of to-day, 8-to-1 concrete was now about equal to 5-to-1 concrete of 20 years ago. The former concrete was more quick-setting than the latter. He had made a test in the last day or two which rather bore upon that point. He took cement which was classed under the British standard specification as of medium setting properties, and he gauged pats of neat cement, 3 to 1, and 8 to 1 of sharp, clean, Thames sand. He found the setting periods were approximately, for neat cement 2 hours, for the 3-to-1 pat 4 hours, and for the 8-to-1 pat 11 hours. There was no doubt that with varying proportions of the aggregate in concrete the relative rate of setting of the whole mass would vary. Of course, apart from the question of grinding, certain qualities of clay, if used in the manufacture of Portland cement, would give a very quick-setting product. There were a few factories where gault was used, and manufacturers who used that clay had to take great precautions to make the resulting cement sufficiently slow-setting to suit users. In connection with the subject of the addition of gypsum to cement, he might give an experience of his own. Gault contained a considerable proportion of gypsum

Mr. Carey.

Mr. Carey. and near the surface of a deposit of such clay the gypsum crystals could be seen quite thick. If that clay was used, and the top spit was allowed to be added in manufacture, the cement produced was of course very heavily charged with gypsum, and the results were of an alarming character. He had found that briquettes of cement manufactured in that way disintegrated after 4 or 5 weeks—went to powder in fact—and the concrete itself swelled, became fissured, and disintegrated. He would counsel the greatest caution in any extension of the practice of allowing gypsum to be added to cement for slowing purposes. There was just one other question he would like to ask the Author. In his notes on the equipment, he described the 4-ton baggage-cranes as having one long and one short leg, with their gantries travelling on a single rail on either side. Judging by the section, the gauge must have been nearly 50 feet. Mr. Carey would like to know whether any difficulty had been experienced from the tendency of those cranes to set across the road.

Mr. Shelford. Mr. FREDERIC SHELFORD asked if the Author could give the cost of the concrete. Although at Folkestone it was not possible to see under the water, there was no doubt that the work there was as good as it was above. At Famagusta, in Cyprus, where he had been associated with Messrs. Coode, Son and Matthews in certain works, he could see to the bottom, about 18 feet in depth, and the work under water was seen to be absolutely in line in the same way as it was above. The machinery and plant at Folkestone had been spoken of as expensive and perhaps extravagant, but he was quite sure it had all been required. Messrs. Coode, Son and Matthews, however, were perhaps a little inclined to standardize their machinery and plant; because he had seen what might be called small editions of the plant in use at Folkestone, used on smaller works in other parts of the world, where perhaps such plant was not altogether necessary. He thought further information on one or two points in the Paper was desirable. One point was the working of the faces of the blocks, by which he understood the Author to mean that the workmen, in putting the concrete into the boxes, worked the fine stuff to the face. As the blocks would not show, he did not quite see the object of that operation. It seemed to him rather to spoil the contact between the blocks, and was perhaps inclined to weaken the heart of the block. The Author also mentioned that the piles, which were of pitch-pine, only lasted 18 months. Probably they were put in in their natural state, untreated with creosote or other preservative. If they had been treated, he had no doubt they would have lasted longer. He had a

piece of wood that had been under water for 4 months, as part of a pile, in the tropics, and it was nearly all holes. On the other hand, a piece of creosoted wood that had been in the water not much less than 12 years, was as good as the day it was put in. With regard to the storage of cement, he would like to know how long engineers would have to go on having to lay out cement and keep it for a month or more in order to render it fit for use. He would have thought that manufacturers now ought to be able to turn out cement which could be used at once. On harbour-works, of course, it was easy to arrange for aerating it, but on works where it was used in small quantities it ought to be usable forthwith. There was one important point of interest to civil engineers. Whoever had to pay for the work were wise in employing consulting engineers of the standing of Messrs. Coode, Son and Matthews. On visiting seaside places on the east coast and south coast during the last 18 months he had been horrified to see at many of them large blocks of concrete on the beach, which once formed parts of sea-walls. Sometimes a few sticks were seen in the sea, which were the remains of a pier. All that represented considerable expenditure which had been almost entirely wasted, owing to the non-employment of consulting engineers. Mr. Shelford.

Mr. H. K. G. Bamber remarked that probably a greater advance had been made in the improvement of the manufacture of Portland cement during the period 1897–1905 than at any other time since the material was first introduced 50 years ago. It might be that from 1897 up to 1900 it was necessary for the Author to lay out his cement and turn it over, at a great cost to himself and others, in order to produce a sound article for his work. Before 1897 improvements in the processes of manufacture were almost entirely wanting, but since that date great advances had been made. It was quite safe to say, as Mr. Blount had said, that the manufacturer could to-day offer engineers cement which was perfectly sound and fit for use in the work immediately on delivery. He gathered from the Paper that the object in turning over time after time—in one case up to twenty times—had been to get a cement that would stand the boiling-test. It was quite easy with the new British Standard Specification, which had been brought into existence during the last 2 years by a Committee under the Chairmanship of Sir William Matthews, to discover whether the cement as delivered to the work would stand the boiling-test. If it would stand that test immediately there was surely no necessity to spend money on turning it over five or six times and keeping it in store 5 or 6 months. At the Mr. H. K. G. Bamber.

Mr. Bamber. present time manufacturers could not understand why engineers went to such heavy expense in turning over cement. The principal improvement that had taken place in the manufacture of cement had been the introduction of the new process of burning, and he would like to make a few remarks on that which might be of interest, in respect of the proportion of lime which was contained in what was now known as rotary cement. It was, he believed, imagined that all rotary cement must necessarily contain a higher percentage of lime than was usually found in the cement manufactured by the older processes; but that was not the case. It was true that rotary cement showed on ultimate analysis a percentage of lime that was somewhat higher than the 62 per cent. which had been the limit specified by many engineers in the past; but probably engineers were not aware that in the older process the raw materials were mixed in such proportions as to obtain a cement clinker the actual analysis of which would show 64 to 65 per cent. of lime. It might surprise some people to know that at the present time, at works where the rotary cement was being manufactured, the proportion of carbonate of lime added to the raw materials was 1½ per cent. less than in the case of the older processes. The anomaly was explained in the following manner. By the more modern process the ash in the fuel used for calcination was eliminated almost entirely from the clinker and passed away with the flue-gases; whereas in the older processes the dried raw materials were interstratified with the coke in the kiln, and when burned out the clinker was mixed mechanically with the whole of the ash in the fuel that had been used for calcination. As it was impossible, in drawing the clinker from the kilns, to separate that ash from the clinker, the ash was mixed up during the course of grinding, and being siliceous material containing very little lime, it had the effect of reducing the percentage of lime in the ultimate cement to about 60 per cent. That was proved by the fact that if from an ordinary intermittent kiln portions of clinker that had not been contaminated with the ash from the fuel—pieces of dried material which had practically retained their shape during calcination but had been well and properly burned—were selected, separated from the dusty part, and completely analysed, it would be found that the percentage of lime in that portion of the clinker was 64 to 65 per cent. Was it reasonable that objection should be made to the more recent cement, the "rotary" material, containing anything between 62 and 65 per cent. of lime, on the assumption that it contained an excess of lime, when the cement manufactured formerly by the older processes was a mixture of clinker containing 64 to 65 per

cent. of lime with a mixture of foreign matter of unknown composition, namely, the ash from the fuel. It seemed to him very much safer that the engineer should use the cement which was absolutely pure and contained a known percentage of lime than that he should use one which, on an ultimate analysis, would appear to show a smaller, but which actually contained a considerably larger percentage. He would like also to say a word with reference to gypsum. In small proportions gypsum had no detrimental effect upon the concrete, and he agreed with Mr. Blount that up to 1 per cent., or perhaps a little more, probably no effect was ever likely to arise; but in proportions greater than that it was very doubtful whether in years to come some defect might not make its appearance. Manufacturers had had to deal with that subject recently, owing to the fact that many engineers had absolutely refused to have any gypsum in the cement at all, and in approaching the matter the manufacturers had attempted to deal with it on natural lines. It was well known that when cement was laid out in a warehouse and turned over it would not absorb any carbonic acid from the air until it had absorbed a certain amount of moisture, and during late years the manufacturers had attempted to assist Nature in that respect, in the final process of manufacture, by submitting the cement in course of grinding to a moist atmosphere, so that when it left the mill it had practically obtained that amount of moisture which it would have required some weeks or months to obtain in the warehouse. In that way the manufacturer had done his utmost to meet the views of the engineer, and had produced a cement of slow-setting character with as small a proportion of gypsum as possible, and if necessary without any at all. With some cements it was not possible to obtain the actual results required entirely by the steaming method. It was necessary, in addition to that method, to add very small percentages of gypsum, not exceeding 0·5 to 0·75 per cent. With regard to the wash referred to by Mr. Blount, Mr. Bamber remembered taking charge in 1897 of works where the chalk and clay were mixed in such a condition that particles of chalk passed through the machinery from the wash-mills to the reservoirs of a size equal to that of a filbert. At the present day the whole of the chalk and clay was ground to such a fineness that 95 per cent. of it would pass through a sieve having a mesh of 32,400 holes per square inch. On the question of gypsum, some very useful information had been presented in a Paper read by Professor Le Chatelier, of Paris, at the Berlin Congress of the Mr. Bamber.

Mr. Bamber. International Association for Testing Materials, in 1906. In that Paper[1] Professor Le Chatelier went very fully into the action of gypsum, more especially in sea-water, and the conclusions he appeared to have come to were that with about 1 per cent. there was practically no danger to be feared, but that with any quantity in excess of that there might be danger ahead, especially with cements containing a low percentage of alumina. Mr. Carey had referred to gypsum in gault. In one of the works of which Mr. Bamber had had charge there was a gault clay-bed, and it had not been his experience to find layers of gypsum at the top of the clay. He would like to ask whether Mr. Carey could say that it was usual, or whether his remarks applied only to one particular locality.

Mr. Carey. Mr. CAREY replied that he had found gypsum frequently on top of the gault.

Mr. Fitzmaurice. Mr. MAURICE FITZMAURICE remarked that the views of Mr. Bamber and Mr. Meik were diametrically opposed. Mr. Bamber would naturally take the rosiest view about rotary cement, while Mr. Meik took rather a darker view. No doubt the actual facts in regard to rotary cement were at the present time about half-way between the two. Probably when further improvements had been made in rotary cement the facts would incline still more to the side of Mr. Bamber. Mr. Fitzmaurice, like Mr. Meik, had also found some little difficulty with rotary cement, and had found it necessary to get rid of some cargoes of it, which in no way approached the test that the Engineering Standards Committee had fixed with regard to expansion, the Le Chatelier test. Possibly if the cement had been kept for a long time and turned over—although he understood from the statements of previous speakers that it did not require turning—it might have become right; but where ground was rather valuable, as in London, it was not always possible to keep cement lying in bins for a considerable time. He did not wish to say a word against rotary cement, as everyone had to use it; but he thought it had not at the present time the same uniformity as was found in the old cement. No doubt it would be very much improved in that respect, and he was sure the makers were taking every possible precaution that rotary cement should have the good qualities engineers were accustomed to, with regard to soundness and, particularly, expansion. The Author had made some very careful experiments and observations while the work was going on, and Mr. Fitzmaurice hoped that other resident engineers would not be

[1] H. Le Chatelier, "On the Behaviour of Cements in Sea-Water."

deterred, by the remarks of previous speakers, from making experiments with cement on other works, as not only were the results useful, but also it was a good thing for engineers who were on works to make such tests for their own benefit. He hoped the Author would be able to do what Mr. Blount had suggested, namely, state what he had found to be the actual increase in volume due to turning, which seemed to vary a good deal. The Paper mentioned six turns, ten turns, and even twenty turns in one exceptional case. At present the Author had only stated the increase in terms of the cost of turning; but as he did not give the cost of turning, the problem presented one of those beautifully simple equations with two unknowns, which were quite incapable of solution. With regard to *Figs. 8* (p. 56), which gave long-time tests of cement with a slight adulteration of gypsum, he could not venture to put his opinion on the point against Mr. Blount's, but he would like to know whether a decrease in tensile strength of 400 lbs., practically 45 per cent., between the end of the second year and the end of the third year, was usual in this class of cement. Mr. Blount had stated that that was quite immaterial, but it seemed to Mr. Fitzmaurice rather a large decrease, and he did not know where it would stop. It would be interesting to know if it really was a serious matter. The strength of the neat cement at the end of the third year was exactly the same as that of the 3-to-1 mortar. With reference to the difficulty of cement setting quickly, the Author practically said that finely-ground cement, with its setting properties regulated by the addition of gypsum, might after a few years develop serious defects; while on the other hand, a finely-ground cement, unadulterated with gypsum, might be dangerous owing to its quick-setting properties. If that were true—and he thought a good deal of consideration had been given to the question by the Author—the engineer was in a dilemma, because whether the finely-ground cement was adulterated or not it was dangerous. The "man in the street" reading the Paper would probably ask himself whether engineers and manufacturers were not now going rather too far in the fine grinding of cement, and whether the limit of fine grinding had not been reached or even exceeded. It was rather treason to say so, but Mr. Fitzmaurice was not at all sure the man in the street would not be right. Mr. Fitzmaurice.

Mr. MAURICE F. WILSON was very much interested in the Paper, having carried out almost similar work at Dover. It had been very interesting while these works were proceeding to observe the small differences in detail with which the works at Folkestone were being carried out. With regard to the cement, he had made a number of Mr. Wilson.

Mr. Wilson. experiments at Dover and was of much the same mind as the Author. Mr. Blount had said that the boiling-test was a good one and that manufacturers were using it a good deal. He himself thought it was an excellent test, but he did not think the Author's tests had been quite severe enough, because he had boiled his briquettes when 7 days old. Many experiments had been made at Dover, and it had been found that a pat was more sensitive to the boiling-test than a briquette of the same age, and also that the longer the pats or briquettes were kept, the more boiling they would stand before showing any sign of cracking. Comparing them with the Le Chatelier test it had been found at Dover that pats boiled 7 days old were about equal to the Le Chatelier test of 40 millimetres; similarly, on boiling 2 days old they were equal to the Le Chatelier test of about 24 millimetres. It had ultimately come down to boiling them 24 hours old simultaneously with the Le Chatelier test, and it had been found that pats which showed very slight signs of cracking were about equal to 9 or 10 millimetres Le Chatelier test. So that virtually pats boiled 24 hours old, if they showed no signs of cracking, might fairly well fix the cement as being passed by the Le Chatelier test. Boiling the pats was a very simple way of testing and required no special apparatus. A number of experiments had been made with regard to aeration, similar to those of the Author, and with much the same results. One or two questions had been asked with regard to the increase in bulk, and the Author would no doubt give the results of his experiments. At Dover it had been found, as the average of a large number of experiments, that five turnings of cement reduced the weight per bushel about 2½ lbs., equivalent to an increase in bulk of a little over 2 per cent. He thought engineers were still rather in doubt on the subject of turning. He did not think they could accept what Mr. Blount and Mr. Bamber had said as to turning being absolutely unnecessary. He knew of a recent case where pats of rotary cement showed considerable cracking and the cement had to be turned three or four times before it could be used. He did not think at present it was safe to say definitely that cement should not be turned. A good many experiments had been made with regard to gypsum at Dover, on much the same lines as those at Folkestone. In every case cement had been taken which was supposed to have had no gypsum added after burning, and the general result had been that in tensile strength the briquettes gained up to 2 per cent. and then began to fall off. The more important thing was the setting under water. Pats or briquettes put under water and allowed to set

had shown signs of cracking almost immediately if the cement contained more than 0·5 or 0·75 per cent. of gypsum. Some pats and briquettes had been tried with larger amounts up to 6 per cent. and after a few days in the water they had crumbled away; so that there was some doubt as to the safety of adding gypsum—at any rate in any considerable quantity. Mr. Wilson.

Dr. J. S. OWENS remarked, with reference to the Author's experiment upon the setting of dry concrete under water, that whether the concrete would set efficiently under water or not, it was certain to set porous, and for work which was on the foreshore and subject to the percolation of sea-water, that was well known to be especially dangerous. Many of the old Case groynes first built were put down with almost dry concrete, and, on taking up the foundations he had found many of them quite rotten, with a deposit of white powder, probably magnesia, mixed with the concrete. At the bottom of p. 68 the Author referred to the fact that when there was any swell at all a large amount of sand and shingle came into the harbour. In August, 1907, Dr. Owens made some experiments on the coast of Norfolk, and he had had the honour of reading a Paper on them before the Royal Geographical Society's Research Department. Some of the results might be of interest to the meeting. The experiments were directed towards determining the size of stone which a current of a certain velocity would move upon a smooth bottom, and some curious results had been obtained. One was that, at a velocity of 0·85 foot per second, sand began to move—that was, sand in the mass—in the form of ripples, and it continued in the form of ripples until a velocity of 2·5 feet per second was reached. At that velocity the ripples were swept away and the movement took place in the form of eddying suspension along the bottom. All the experiments which were tried between those two velocities showed the following peculiarity. A stone never rolled far; it never rolled, in fact, beyond a ripple, and it was always deposited in the trough of a ripple. At the velocity of about 2·5 feet per second the current therefore suddenly acquired a power which it did not possess at any previous velocity, namely, the power of moving particles of shingle. After that velocity the particles rolled continuously. He was speaking of current alone, and did not refer to wave-action at all. Between the two velocities the current had no power of moving particles of stone or shingle—they were entirely masked by the sand. Further, when the current was capable of moving a large stone 4 to 5 inches in diameter on a sandy bed, anything which checked the movement of the stone Dr. Owens.

Dr. Owens. invariably resulted in the sinking and burying of the stone in the sand. Those things put together had led him to infer that sand, when present in quantity, masked entirely the movement of shingle; and that, he thought, was the experience of most engineers who had had to do with sea-defences; for instance, when material was being eroded from the seashore, sand moved away first, and the shingle was left behind. Again, when observing the movement of a stone about 2 inches in diameter, he found that it rolled along with a current of about 2 feet per second. But he was struck with the peculiar fact that the material over which the stone rolled was small shingle ½ inch to ¼ inch in diameter, which was at rest. He took about 6 or 7 lbs. of shingle and threw it into a current of about 3 feet per second velocity, which current had previously rolled along a stone 4 inches in diameter. The current was unable to carry away that fine shingle, which only moved very slowly from the edges. Therefore, it appeared to him that the power to move shingle or stones of any kind varied with, among other things, the number of stones present—whether it was the case of a single stone rolling along a smooth bottom, or a mass of stones. The mass of stones or shingle required about four times the velocity to move them which a single stone of the same size required; and that held good in all cases in which he tried it. To go a little farther—although his experiments did not cover this point—he thought that beyond the velocity of 2·5 feet per second sand-ripples were swept away, but that it was probable that sand-waves of an entirely different kind were formed—large waves 14 to 15 feet in length, and perhaps 6 to 7 inches high. At any rate, a photograph of the Goodwins, or of any other place over which such a current was flowing, would nearly always reveal an irregular wave-like bottom. Therefore he concluded that the same law would hold good above the velocity of 2·5 feet per second, that was, that the sand-waves would entirely check the travel of shingle. He did not pretend to have gone very far with the subject, but it seemed to him that the experiments indicated that the movement of material was a very peculiar thing, and not at all on the surface. Although a current of, for instance, 0·85 foot per second would set sand in motion, the theoretical current which should move sand-grains singly was only about 0·21 foot per second. He had tried to deduce a formula for the size of stone which a given current would move, and taking the diameter of the stone in inches and the velocity in feet per second, the nearest approach he could get was $d = \frac{v^2}{2 \cdot 2}$ for flints.

The PRESIDENT observed that there were one or two points arising out of the discussion on which he would like to say a few words. The first point related to wave-breakers. Of course, in the re-entrant angle formed between the old groyne and the base of the work, a very heavy sea fell on the occurrence of south-westerly gales, and no engineer, unless it was unavoidable, would form a re-entrant angle or have one at the root of his work. However, in the case of the Folkestone pier it was absolutely unavoidable, and it had therefore become necessary to absorb the force of the sea in that particular angle, which had been done by the provision of the mass of pell-mell blocks. Many years ago he had occasion to make some experiments with regard to the percentage of voids in heaps of solid blocks. He had some model concrete blocks made and put together in different ways, with a view to ascertain what the ratio of the voids was to the solid mass, and he found that it ranged from 33 per cent. to 25 per cent., according to the formation and distribution of the blocks. Therefore the spaces available for absorbing the sea falling on to the blocks in that re-entrant angle amounted to between one-fourth and one-third of the total space occupied by the heap of blocks, and it was in those voids, instead of in the openings in the base of the old work, that the sea was absorbed when it fell into the angle. With regard to the interesting point which Sir John Wolfe Barry raised, as to the travel of the shingle from west to east; before the new work was started the harbour of Folkestone had to be kept open, and, as a matter of fact, the old pier stopped all the shingle that passed along the front. He had had occasion to go into the matter, having been called upon to report on the sea-defences, before he had anything to do with the Folkestone pier, and he was quite prepared to say that before the works were touched or extended no shingle passed Folkestone harbour. In fact, it could not pass the harbour without blocking it. The shingle was stopped, and hence the very large mass of shingle that had gathered to the west of Folkestone pier. With regard to the protection of the length of sea-front between Folkestone and Dover, to which Sir John Wolfe Barry had also referred, the same protective agency that had been in operation for many years would continue, the travel of shingle along that length being due to the wastage of the cliffs between Folkestone and Dover only; hence it was that, to his knowledge, on the west side of the Admiralty Pier at Dover there had been practically very little accumulation for the last 20 or 30 years. This showed that the travel between Folkestone and Dover was extremely small. Another point was that in recent years authorities had been protecting their foreshores to the westward, at Sandgate, The President.

The President. Folkestone, and various other places, and the groynes and sea-defence works they had put up had trapped the beach and prevented it from coming to Folkestone, thereby reducing the quantity of beach to the west of the work. He thought the new work that had been carried out at Folkestone had not affected, and would not affect, the sea-defences between Folkestone and Dover. Another question alluded to by Sir John Wolfe Barry was why the money had been expended at Folkestone. The works were started at Folkestone in the old days of keen competition between the South-Eastern and the Chatham and Dover Railways, Folkestone being the South-Eastern port and Dover the Chatham port. Now there was a union between them, and the conditions were not the same as when the works were commenced and during the earlier period of their progress. Although not strictly relevant to the Paper, he would like to record the fact that he had been informed many years ago, by the late Mr. Henry Lee, the contractor for the original Admiralty Pier at Dover, that Portland cement was first used on that work in 1849. The concrete blocks were then 3 to 7 tons each in weight. Before that date the concrete for the blocks was made from lime and pozzolana instead of cement.

The Author. The AUTHOR in reply, thought it might have been preferable if Mr. Dyce Cay had investigated the conditions prevailing at Folkestone before stating his opinion regarding the applicability of the methods adopted at Lerwick to the pier under discussion. A very slight consideration of the traffic-requirements described in the Paper, and a glance at a chart of the English Channel, and at the Plate accompanying the Paper, would have shown him that the conditions were so entirely different at the two places as to make a comparison between them ridiculous. Lerwick pier was a shallow-water pier in a land-locked harbour where the greatest fetch was rather less than 2 miles. There was nothing that could be termed a sea striking against it, the greatest disturbance being only a "lop" or "jobble," with waves not more than 3 feet high. If it were otherwise, the wooden shed which stood upon the pier, without any protection whatever, would have been destroyed. Folkestone, on the other hand, was a deep-water pier standing in a maximum depth of water of 60 feet. It was situated on an open coast exposed to a fetch from the south-west of 120 miles, and was subject to the stroke of waves measuring 13 feet from trough to crest. The design advocated by Mr. Dyce Cay would have been impossible to carry out at Folkestone. The seas would have played havoc with the side walls, and the rubble hearting would have been drawn down by the waves and distributed over a large area. Even with the 20-ton blocks used in the con-

struction of Folkestone pier the scar-end had suffered damage during heavy gales. On one occasion three 20-ton blocks were carried away. It might be imagined what the effect would have been if the pier had been built on the lines which Mr. Cay had suggested. In his remarks regarding staging, Mr. Cay paid a poor compliment to the intelligence of those who had used and were using staging in the construction of breakwaters. The method of construction by means of barges as adopted at Lerwick might possibly be economical in such a sheltered situation, though in the Author's opinion a light staging would have been the means of executing better, quicker, and cheaper work. The Author had had considerable experience in both methods of construction, and he maintained that to suggest the barge method for the construction of a deep-water pier on an exposed coast, where there were few days of absolutely still water, was absurd. Mr. Cay had also referred to the narrowness of the platform. The width which he mentioned occurred at the outer end of the Knuckle Station, which was a secondary one. The bulk of the traffic was carried on at the new pier-station at the root of the pier, where the platform was 20 feet wide for about half its length, and 15 feet wide for the remainder. As regarded the wave-breaker, the Author agreed with Mr. Dyce Cay to a certain extent. There was bound to be a certain amount of wastage, unless the wave-breaker was composed of blocks of sufficient weight to be immovable during the heaviest seas which acted on them, and also unless it was on a rock foundation where no settlement could occur. Without these two conditions a pell-mell wave-breaker might have to be fed from time to time, and this might be done in a simple manner by building one or two blocks of concrete in situ. Mr. Hurtzig had drawn attention to the battered face of the piling on the East Face Renewal. When that part of the work was being designed the engineers gave particular attention to this point. It was considered desirable that a vertical face, such as existed at all the other berths, should be provided if at all practicable. It was found, however, that owing to the blocks which projected from the face of the old pier, to the rail panelling referred to in the Paper, and to the debris which had accumulated at the foot of the pier, it was impossible to provide a vertical face without making such a considerable break in the line of the pier as would have been a serious drawback to steamers coming in to take up their berths. After careful consideration it was decided to minimize the break in the coping-line as much as possible, and this could be done only by driving the piles with a batter. The vertical face would have been preferred,

The Author.

The Author. not only because a battered face threw farther away from the pier a paddle-steamer which was already farther away from the cranes than a screw-steamer by the width of its sponsons, but also because, should a paddle-steamer alongside a battered face roll to any serious extent, there was danger that the floats might be injured. The batter of 1 in 12 was not likely to cause any danger to the floats from the amount of rolling which took place at Folkestone. The wear of the fenders on a battered face was more than on a vertical face, which was another reason for preferring the latter. As regarded the depths to which the piles had been driven, Mr. Hurtzig had overlooked some of the loads which they had to carry. The piles carried the whole weight of the deck, with its load of the baggage-crane, locomotives, etc.; but beyond the question of loads there was good reason for driving the piles well into the greensand, namely, to prevent any possibility of sea-action undermining the pier, and also to provide for any future deepening that might be considered necessary. Sir Whately Eliot had given some interesting ancient history regarding Folkestone, which confirmed generally the opening remarks in the Paper. With regard to his opinion as to the cause of the dilapidated condition of the old pier, there could be no doubt that one of the main causes was the fact that the blocks had not been carried down to a secure foundation; but that defective concrete had also contributed largely to the destruction of the old pier the Author could testify from under-water examination made by him, when he found some of the old concrete in the cavities under the pier to be so soft that it could be torn away in handfuls. Sir John Wolfe Barry in his interesting remarks had compared the accommodation on the English side of the Channel with the accommodation at Calais, much to the disadvantage of the former. The two cases were not at all parallel. The accommodation at Dover and at Folkestone had had to be stolen from the sea and was provided on piers built in deep water and exposed to heavy seas. At Calais and Boulogne, on the other hand, the accommodation was provided on quays situated on the banks of creeks snugly sheltered from the sea, where there were acres of land available. The remarks which Sir John had made regarding the probability of Folkestone pier acting as a huge groyne in stopping all the shingle which would otherwise have drifted towards Dover, had been dealt with by the President; but the Author might say that just before the completion of the works a very careful under-water examination had been made to ascertain whether any shingle was accumulating against the west side of the pier. Nothing had been found but sand, which had been proved by soundings to be almost identical in level with

the sea-bed levels taken before the work was commenced. The probability was, as the President had pointed out, that any overflow from the stone groyne passed round the pier into deep water. Mr. Carey did not appear to have understood correctly what was meant by the "creeping" of the blocks in the foundation-course, referred to in the Paper. It was not that there was any actual movement of the blocks, but merely that in setting them it was found impossible to maintain the position of the joints exactly as shown upon the drawing. It would be impossible to say with any degree of exactitude where the joints between bags would be in a pier built with a substructure of sack-blocks as employed at Newhaven, and the comparison between that system and the block-work system with reference to "creeping" did not apply. The reply to Mr. Carey's inquiry as to whether the briquettes made for cement-tests had been gauged with fresh or salt water was, that though the tests of which the results were given in the Paper had been gauged with fresh water, the Author had made a considerable number of experiments with unadulterated cement gauged both with fresh and with salt water, and in every case he had found that there was a uniform increase of strength, extending to the end of the tests, which in some cases lasted over a period of 3 years; the only difference being that by using salt water the setting was slightly retarded while the strength was rather higher than with fresh water. With regard to the same speaker's question about the baggage-cranes, the outer crane had a span of 44 feet, and with it no tendency to set across the road had been experienced; the inner baggage-crane had a span of 73 feet, and though there was a slight tendency to set across the road when travelling with the crane at either end of the gantry, the difficulty was very slight. It was easily overcome by squaring the gantry and running the crane to the middle of the span before travelling. With reference to Mr. Shelford's suggestion that the piles might have been creosoted with advantage, the piles as delivered on the works were about 100 feet long and 18 inches square, and it was considered unnecessary to incur the expense of creosoting. With regard to the criticisms which had been offered on the aeration of cement as dealt with in the Paper, it would appear that now that the rotary kiln was so much in vogue the remarks in the Paper might seem to some rather antiquated. The Folkestone cement had been manufactured in the old type of draw-kiln and had undoubtedly required the aeration it received. The Author's hope that his remarks on cement adulterated with gypsum would evoke a discussion in which some reliable information would be given regarding The Author.

The Author. the behaviour, over a long period, of cement so treated, had not been fully realized. Mr. Meik had given some results of interesting experiments which he had made, and they confirmed to a certain extent the uncomfortable results which the Author had found from the tests made at Folkestone, but, like the Author's, Mr. Meik's tests covered too short a period to be conclusive. Mr. Meik did not see how engineers were to do without gypsum for controlling the setting properties, and in this the Author agreed with him as long as the very finely ground cement which was now on the market continued to be manufactured; but the danger appeared to be that by using gypsum a simple and easy method of getting out of the present trouble was being taken without due regard to the future. It was asserted by Mr. Blount and Mr. Bamber that cement adulterated with a moderate proportion of gypsum would have no detrimental effect upon the concrete. This might be so, but in the Author's opinion it could be proved only by tests extending over 5 years or more. Several speakers had referred to tensile tests of neat cement showing a decrease in strength with age; but no particular age was given. The Author regretted that he had not stated more clearly that the diminishing strengths referred to in the Paper were quite exceptional at Folkestone. The quantity of cement used on the Folkestone works was upwards up 30,000 tons, delivered in 340 consignments. Each consignment was tested, and many of the tests extended to 3 years; but in no instance, save in the case of the adulterated cement, had there been a downward tendency at the end of the period. There were irregularities frequently in the diagrams round the 4 months to 6 months period, and in one case there was a drop to 2 years; but in every instance except that of the adulterated cement, there was a recovery to 3 years. That the use of sand in cement had a steadying influence on the results of tests, there could be no doubt; but the Author had noticed irregularities also in mortar-tests coincident, though less in degree, with those in neat-cement tests, and although Mr. Blount, Mr. Dyce Cay and others might say that there was no danger to be feared when sand was used, yet when the cement which was applied for the very purpose of binding the sand together was found to be diminishing in strength, it seemed natural to infer that the strength of the mortar as a whole must deteriorate. The valuable experiments carried out at Dover by Mr. Wilson were strong evidence that the addition of gypsum to cement which was to be used in sea-work was distinctly dangerous. Mr. Fitzmaurice had gone to the root of the matter in implying that the fine grinding was being carried too far. The Author's opinion was that it

would be safer to use a cement ground finely enough to be of assistance as regards soundness, and yet not so finely ground as to be inconveniently quick-setting. These conditions were obtained during the early days of the Folkestone works when cement briquettes, which set hard in 20 minutes or longer, frequently passed the boiling-test when 2 days old. Mr. Bamber was wrong in thinking that the twenty-two turns mentioned in the Paper were for the purpose of getting the cement to pass the boiling-test. More than five turns had seldom been required for this purpose, and the twenty-two turns given to the particular consignment mentioned were in the nature of an experiment to ascertain how long the increase in volume would cover the cost in turning. The increase in volume due to six turns was approximately 3½ per cent., which, with cement at 30*s.* per ton, was equivalent to 1*s.* per ton, and this was almost exactly the cost of turning six times. With regard to blockmaking, Mr. Shelford would evidently prefer the blocks to be left with rough faces. Contractors would no doubt have much sympathy with this view; but in the Author's opinion it was not a wise suggestion. The working of the faces of the blocks did not affect the concrete more than 1 inch from the face and could not possibly weaken the centre of the block. The practice had the advantage of producing a face which was practically waterproof, and of preventing the infiltration of sea-water into the concrete of these blocks, which were set below low water without beds or joints. That the mortar obtained a perfectly secure grip of the face of the blocks above water had been shown in one or two instances when cutting had to be done on work which had been built for some time. It was found that concrete, and even granite, would break away before the mortar in a joint would part company with it. The Author had pleasure in supplying the following information asked for by Mr. Blount regarding the cement which was tested to ascertain the increase in volume. The Author.

Lot "A."

Residue on sieve of 2,500 meshes per square inch . . .	0·17 per cent.
" " 5,776 " " " " . . .	2·70 "
" " 14,400 " " " " . . .	8·50 "
" " 32,400 " " " " . . .	21·00 "

	As delivered to Works.	After 22 Turns.
Increase in volume	..	9 per cent.
Weight per bushel	101 lbs.	95 lbs.
Time of setting	5 minutes	25 minutes.
Specific gravity	3·145	3·125
Boiling-test	Sound	Sound

The Author.

Chemical Analysis.

	Per Cent.	Per Cent.
Silica (SiO_2)	21·36	20·88
Insoluble residue	1·26	1·04
Alumina (Al_2O_3) / Ferric oxide (Fe_2O_3)	11·58	11·48
Lime (CaO)	61·46	60·98
(Value after deduction of that necessary for the formation of calcium sulphate)	60·40	59·87
Magnesia (MgO)	1·01	1·02
Sulphuric anhydride (SO_3)	1·52	1·58
(Calculated at calcium sulphate)	2·58	2·69
Carbonic anhydride (CO_2) / Water (H_2O)	0·96	1·84
Alkalis and loss	0·85	1·18

Lot "B."

Increase in volume after the eighth turn	2 per cent.
Weight per bushel	101 lbs.
Residue on sieve of 2,500 meshes per square inch	0·80 per cent.
" " " " 5,776 " " " "	4·70 " "
" " " " 14,400 " " " "	10·75 " "
" " " " 32,400 " " " "	22·90 " "
Specific gravity	3·170
Time of setting	8 minutes.

Chemical Analysis.

	Per Cent.	Per Cent.
Silica (SiO_2)	21·86	
Insoluble residue	1·06	
Alumina (Al_2O_3) / Ferric oxide (Fe_2O_3)	11·90	
Lime (CaO)	61·20	
(Value after deduction of that necessary for the formation of calcium sulphate)	..	60·18
Magnesia (MgO)	1·01	
Sulphuric anhydride (SO_3)	1·45	
(Calculated at calcium sulphate)	..	2·47
Carbonic anhydride (CO_2) / Water (H_2O)	0·70	
Alkalis and loss	0·82	

The particulars given related to two consignments ("A" and "B") of which one maintained a steady increase of volume up to the twenty-second turn, while the other showed no increase at the eighth turn.

Correspondence.

Mr. HERBERT E. ALLEN asked whether the repeated turning of the cement had been done by hand, or by tipping sections of the bin-floor allowing the cement to fall on to the floor below. As it was stated that generally the specified five turns were sufficient, Mr. Allen concluded that the number of turns up to five was covered by the general specification, and allowed for in the cost of the cement or concrete per cubic yard; but when seven turns or more were given to the cement, how had this been dealt with in regard to payment? It seemed evident that it would involve expense to the contractor, unless perhaps the Resident Engineer had had a special staff of men to test and deal with the cement before handing it over to the contractors for use. Again, would the Author say whether the heavy temporary work—staging, piling, overhead cranes, etc., had been met by a lump sum, or by an allowance through the schedule of prices, or whether the contractors' general prices for excavating, block-setting, etc., per cubic yard, had included all temporary work necessary to carry them out? A few details of the cost of the large concrete blocks would be of interest. Mr. Allen.

Mr. H. KELWAY BAMBER asked what had been the increase in bulk of the cement after the tenth and twenty-second time of turning. Had it been found that a bushel of this cement of increased bulk would make as much satisfactory concrete as a bushel of the original cement when not too quick-setting? Mr. H. K. Bamber.

Mr. JOHN S. BRODIE observed that the extensive and interesting tests of the cement showed the value of thoroughly aerating it before using, also the risk of adding gypsum to render a cement slow-setting. It was noticeable that the proportions of shingle to sand and gravel were not carefully gauged; the disadvantage of an ungauged aggregate was the probability of getting a porous concrete. The precautions taken for ensuring accurate work in block-setting under water appeared to have been very elaborate and well devised. It did not appear that the foundations carried into the greensand to a minimum of 2 feet would have been stable enough without the addition of the apron 13 feet in width. Probably a slightly greater depth of foundation would have obviated the necessity for an apron. It was not clear how the shuttering for the mass concrete could cause the blocks nearest the Mr. Brodie.

Mr. Brodie. centre-line of the pier to "creep" more quickly than the outer blocks. The method of jointing and bedding the blocks was exceedingly good, and showed that this important part of the work had received more than ordinary attention. Could not the strengthening of the root of the old pier have been carried out on the same principle as the widening of the old pier on the west side? That would have been more expeditious and cheaper than the block-work carried on steel-cylinder foundations.

Mr. Carron. Mr. F. G. CARRON asked why circular pier-heads were almost invariably adopted: he thought it would be more economical to construct them square in plan with slightly rounded corners. The advantage of straight walls was evident where landing-steps were required at the head. With regard to the construction generally, it would be difficult to criticize any part adversely, with the exception of the method of renewing the face of the old pier. He thought it was a pity that there the construction could not have been of a nature which in permanence would have been more like the rest of the work. It must be assumed that lack of funds had been the principal factor in the choice of greenheart sheet-piling. It would have been preferable to drive reinforced-concrete piles at intervals and to form a mass-concrete wall between them in place of the chalk filling, the foundations of the wall being formed in short lengths, and the wall properly bonded with the old work. With regard to the concrete blocks used as wave-breakers, doubtless the 20-ton blocks were well suited for this position; but a few 60-ton concrete blocks recently used for a similar purpose at a breakwater with which Mr. Carron was acquainted had been moved by gales in a line parallel with the breakwater—two of them creeping along in this way as much as 40 and 50 feet. This proved that they were heavy enough to withstand the force of a wave exerted square to the breakwater, but not heavy enough to resist the resultant of a blow which struck the breakwater at an acute angle. For positions such as this, small rubble deposited as protection to a wall was of little or no use. Another point in regard to concrete blocks was suggested by that portion of the President's recent Address which referred to the use of reinforced concrete as a material for harbour-construction. In some positions great difficulty was experienced in launching blocks large enough for exposed situations without the use of very expensive plant, which it was not possible to provide where the number of blocks to be dealt with was small. In such a case Mr. Carron saw no reason why a reinforced-concrete shell should not be made on shore, launched and put in position, and then filled with concrete. This would be a great improvement on building blocks on the

foreshore in ordinary timber moulds, as it would ensure a more perfect skin to the block, which was a very important point, and it would also avoid the excessive waste of cement due to tidal action on new concrete, as the shell would be practically water-tight. In this way a block of 100 tons weight could be made, whose shell alone would not weigh more than about 20 tons. Mr. Carron.

Mr. R. G. Clark remarked that it was a well-known fact that aeration extended the setting-time, but his experience was that mixing the cement with the aggregate also had this effect. By this he suggested that although the neat cement might show a setting-time of, say, 1 hour, that of the concrete might be about 4 to 5 hours, thus giving ample time for mixing before it had begun to set. Again, the weather affected the cement, and it was of great importance to ascertain the proportion of water to use, as this fluctuated with the weather and with the aggregate used, such materials as crushed brick and hard ballast requiring different quantities of water. The aggregate, again, demanded more than passing attention, as he had found that by decreasing the sand and replacing it with stone he obtained results 50 per cent. higher in crushing strength. It was easily seen that if there was a large proportion of fine sand there was a large area for the cement to cover; and on the other hand, in a block with a fair proportion of stone the area was reduced and consequently the block was richer, although the amount of cement was the same. He felt sure that in many cases engineers might be assisted by the makers of the cement, and at least an interchange of ideas from two different points of view might be beneficial to both parties. With reference to the concrete cylinders, he suggested that reinforced concrete would have been suitable, as the reinforcement could be designed to take any tension that might be set up due to the cylinder canting. He was inclined to think it would be as cheap as the ordinary concrete, as it could be handled easily, and in his opinion was far preferable to the steel shells. Mr. Clark.

Mr. C. Colson observed, with regard to the temporary repair of the old pier by the use of concrete in bags, that it might be interesting to note what was done in this connection during the construction of the Hamilton dock at Malta.[1] There the excavation of the dock was in rock which was badly fissured. Some of the fissures were in connection with the sea; they were of appreciable width, and in some parts widened out into cavities. In order to Mr. Colson.

[1] C. Colson and C. H. Colson, "Hamilton Graving-Dock, Malta." Minutes of Proceedings Inst. C.E., vol. cxv, p. 360.

Mr. Colson. check the great flow of water from the sea into the dock-excavatio it was necessary for divers to stop some of the largest of the fissure and cavities under water. The course adopted was to clear ou the fissure or cavity as far in as it was safe for the diver to go, and then to pack in small bags of concrete as closely and as regularly as possible. The bags varied in size according to circumstances, but were never larger than the diver could handle without undue exertion, and were never filled more than two-thirds; this ensured close and regular packing and consequently reduced the interstices to a minimum, and the inflow of water between the bags was correspondingly affected. With regard to the control of the setting-properties of Portland cement, in view of the present knowledge as to the ultimate effect of gypsum—or indeed of any other adulterating or controlling medium—on the soundness of the cement, all such additions should, in his opinion, be avoided until further experiment and observation removed the doubt which now attached to them. In the meantime, thorough aeration—whether ensured by the ordinary method of bulking and turning, which caused some delay and involved extensive storage, or by the more up-to-date mechanically aided method, which accelerated the process and saved time—would, if properly carried out, give the needed control. As to the soundness of concrete mixed dry and deposited under water in bags, he concurred in the view that soundness under such conditions was very problematical, except when bags of very small dimensions were used; and in any case, owing to the interstices not being so solidly filled by the dry sand and cement as would be the case were the concrete mixed in the ordinary way, the sand and cement would tend to settle, with the result that the lower part would be denser than the upper. Experiments made by him in this connection had not been altogether satisfactory, and had emphasized this tendency to settle. If the Author could give some details of the cost per unit of the chief sections of the work, such as the pier at the BB section (Fig. 5, Plate 1), the blockwork in the strengthening at the west side of the pier at the root, the cylinder foundations, etc., it would add to the value of the Paper.

Mr. Mann. Mr. I. J. Mann considered that some very useful information was derivable from the history of Folkestone Pier given by the Author, particularly as regarded the movement of loose material along the foreshore and the adjoining sea-bottom. In this case a solid structure had been projected across the line of drift, and the result, as might have been anticipated, was a temporary stoppage of the movement of the shingle, etc., in its normal direction, for the whole length of the old pier (about 600 feet). After the lapse of 11 years

the loose material began to pass the seaward end of the old pier and to form a shoal at the entrance to the harbour. He would be glad if the Author would state the depth of water in which the old pier originally terminated. The Author referred to the accumulation of sand and shingle in the area dredged out for the reception of the apron-blocks, and it would be interesting to know if this movement of sand and shingle had occurred at or near the seaward end of the extension. That the effect of waves in current-less water of moderate depth was to produce a simple oscillation of any loose particle on the bottom, could readily be observed in clear water. After each forward oscillation, the particle returned to its original position. The amplitude of the oscillation, *ceteris paribus*, was no doubt directly proportional to the height of the wave, and inversely proportional to the depth of the water; but these proportions did not seem to have been practically determined, which was somewhat remarkable when their usefulness to harbour-engineers was considered. When a tidal current was present, and when the direction of the waves coincided more or less with that of the current, the forward oscillation of the loose particle was assisted, and the backward oscillation was retarded by the current, so that, instead of returning to its former position, the particle was carried back to a position in advance of that which it had before occupied, each successive wave producing a similar effect: in this way a drift or travel was produced in the direction of the prevailing waves which in its turn depended on the direction of the prevailing wind. As regarded the depth to which the disturbing action of waves could be propagated, there were so many conflicting opinions that a thoroughly practical and systematic investigation was extremely desirable, with a view to determine what relation existed between the height of a wave and the disturbance it was capable of producing at different depths, not only at low water when the current was generally at its minimum, but also shortly after half-tide when the current was usually at its maximum. The use of an apron or pavement of concrete blocks to protect the foundation of the pier from being undercut was neat and would fulfil its desired purpose, provided the pavement itself were not undercut. In deep water and where otherwise permissible, a low mound of loose rubble stones averaging 1 or 2 cwt. each had often been found to form a very effective protection against undercutting. The rubble adapted itself to the circumstances more readily than large concrete blocks, for as the sand was gradually scoured away it was immediately replaced by the rubble. In addition to its efficiency, this method had the advantage of being simple and inexpensive. The moderate weight Mr. Mann.

Mr. Mann. of the blocks used in the recent extension was judicious and economical, and an example that could be safely followed; at the same time, it should not be forgotten that the stability or immovability of any one block depended by no means entirely on its own weight, but was greatly increased by the weight of the superincumbent blocks. Having had considerable experience in the use of blocks of excessive weight (about 350 tons), and also of blocks of very moderate weight (about 10 tons), he had no hesitation in saying that excessively large blocks not only added very considerably and unnecessarily to the cost of the work, but also retarded progress, owing to the time and trouble involved in preparing a perfectly level surface of large area in the foundation for the reception of the block; the latter objection being accentuated when the work was in an exposed position, and rendering it necessary that every possible advantage should be taken of summer weather. If the Author would state the height of the waves met with during the course of the work, or the maximum height of the waves at the seaward end of the extension, he would further add to the interest of his Paper. Reference was made in the Paper to the tendency of the foundation-blocks to "creep" forward as they were set, which, if not checked, would have brought two vertical joints over one another. Even if this had occurred, however, it would have been of little consequence, and it would seldom happen. In the breakwater at Karachi all the vertical joints were over one another, which subsequently proved to be an advantage when unequal subsidence took place in the foundation. With reference to the tests employed to ascertain the quality of Portland cement, it was somewhat difficult to understand why the tensile strength of neat cement had ever been introduced; the cement was rarely, if ever, under tensile stress. It was well known that its cohesive strength greatly exceeded its adhesive strength, and no such test had ever been employed in testing any ordinary cementing material, such as ordinary glue or other cement. The cohesive test in the case of neat Portland cement was not only useless but also misleading, inasmuch as the cohesive strength of coarsely ground cement exceeded that of the very finely ground. It therefore seemed almost unaccountable why the comparatively useless test of cohesion should be persisted in, and the infinitely more practical and reliable test of adhesion neglected.

Mr. Tait. Mr. W. A. Tait could not help thinking that the value of the Paper would be greatly enhanced if the Author could see his way to give some particulars of the cost of the whole of the recently constructed work, as well as of several of the more important items. The cost from the commencement must no doubt have been

considerable, but, on the other hand, account should be taken of the Mr. Tait. value to the railway-company of the large area of land which had practically reclaimed itself, and which probably would be free from any complications in regard to title. The Author was also to be congratulated on the very elaborate and continuing series of cement-tests described in the Paper. His remarks on the increase of volume of the cement due to aeration were of great interest. In the case of the Talla waterworks, lately described,[1] the practice had been to pay contractors upon the tonnage of cement delivered, and in this case, of course, the contractor got no benefit from the increase in the bulk of the cement due to its being repeatedly turned over in the sheds. The Author had drawn attention to a noteworthy point on p. 76, where he referred to the continuous sheeting having the effect of raising gauge-piles already driven. This had some resemblance to the disturbance of ground, fencing, culverts, etc., which was occasionally observed a considerable distance away from the leading end of a high railway-embankment in course of formation. He would be glad if the Author could supplement the Paper with some further information as to the rate at which the piles were driven from the commencement to the finish; and he might also add explanatory sketches showing at what time the movement of the gauge-pile was first noticed, and how near the sheet-piling was to it when the movement took place. It was, of course, common knowledge that wet sand was an extremely troublesome material in which to drive ordinary piles. The Author's remarks on the use of concrete cylinders in ground which was not homogeneous quite bore out Mr. Tait's own experience in the sinking of somewhat similar cylinders on the Clyde at the Broomielaw, and it would seem that cylinders of such a type were unsuitable for use in ground containing boulders, old masonry, or timber-work. The Author did not mention what plant was provided for maintaining the air-pressure, referred to on p. 72, when dealing with the agitation of the green-sand; and it would be of interest to learn whether the sand was sufficiently homogeneous and compact to prevent the air from getting away suddenly, and so causing a corresponding reduction in the pressure. The actual pressure near the nozzle might also be stated. No doubt, from the fact that two divers were necessary to control the nozzle, there could not have been great loss of pressure between the receiver and the nozzle.

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[1] W. A. P. Tait, "The Talla Works for the Water-supply of Edinburgh." Minutes of Proceedings Inst. C.E., vol. clxvii, p. 102.

Mr. Thompson. Mr. J. HANNAY THOMPSON was very much interested in the information given by the Author with regard to the addition of gypsum to cement. About 7 years ago he made tests of cement which, having been first analysed and certified to be free from gypsum, had quantities of gypsum varying from ½ per cent. to 3 per cent. added to it. The results of the neat tensile tests showed that at 7 days and 28 days there was an increase of strength over the unadulterated cement of about 100 lbs. per square inch, which gradually increased until at 9 months the strength of the adulterated cement was about 150 lbs. per square inch higher than that of the unadulterated cement. At 12 months there was a fall in strength of 50 lbs. to 150 lbs. per square inch. The results of similar tests made with 3-to-1 standard sand showed that cement to which ½ to 1 per cent. of gypsum had been added did not give such high tensile results as the unadulterated cement at 7 days to 12 months, but cement to which 1½ to 3 per cent. of gypsum had been added gave results about 50 lbs. per square inch higher than the unadulterated cement. The following Table showed the results of some tests of the final time of setting of cement to which different percentages of gypsum had been added, the tests being made at intervals of 3 days, after the gypsum had been added.

Number of Days after Addition of Gypsum.	No Gypsum.	0·5 per Cent. Gypsum added.	1 per Cent. Gypsum added.	2 per Cent. Gypsum added.	3 per Cent. Gypsum added.	Temperature ° F.
3	10 minutes	20 minutes	4¾ hours	8 hours	10 hours	73
6	20 "	27 "	43 minutes	48 minutes	8 "	74
9	20 "	19 "	25 "	4 hours	7 "	72
12	11 "	36 "	35 "	4 "	4½ "	73
15	10 "	1 hour	1 hour	5 "	7 "	72
18	10 "	2 hours	3½ hours	6½ "	6¾ "	70
21	10 "	2½ "	3½ "	5½ "	5 "	71
24	10 "	2½ "	3¾ "	5 "	5 "	72
27	10 "	4¼ "	4½ "	5 "	5¼ "	73
30	10 "	4¼ "	4½ "	5 "	6 "	70

It would be noticed that, with an addition of 1 per cent. of gypsum, the time of setting, 3 days after mixing, was 4¾ hours, and that at 9 days the time of setting was reduced to 25 minutes; while corresponding results were shown with an addition of 2 per cent. and 3 per cent. of gypsum. Generally the results of the tests appeared to show that the addition of ½ per cent. of gypsum, 27 days after

mixing, had practically the same effect as the addition of 1, 2, or 3 per cent. Pat-tests were also made, the cement being gauged with fresh water. Some of the pats were put into fresh water, and others in salt water, both before and after setting. The unadulterated cement was sound under all the tests, but pats made from the cement to which ½ to 3 per cent. of gypsum had been added all failed to pass these tests. The pats to which 1½ to 3 per cent. of gypsum had been added were very badly cracked in all cases, were quite soft, and had a scale on the top; and there was a white deposit both on the cement and on the glass. These tests were made immediately on the cement being received on the works, and after it was bulked. The tests were repeated after the cement had been aerated for 5 weeks. The same percentage of gypsum was added in each case, and all the pats stood this series of tests quite satisfactorily. Boiling-tests were made of cement adulterated in the same way. The tests were made 2 days, 4 days, 7 days, and 28 days after gauging, and in every case the adulterated cement stood the boiling-test, although it had failed under the water-pat tests. Cement often stood the pat tests in water, but failed under the boiling-test. In the above case, however, the addition of gypsum caused the reverse to happen. Mr. Thompson.

Mr. Voisin, of Boulogne-sur-Mer, considered that the Paper was the more worthy of attention in that it accentuated again one of the most noticeable characteristics of English sea-works, which consisted not only in the care given to the arrangement of the work-yards, but above all in the selection of plant of sufficient size and strength to enable the works to be carried out with the minimum delay through the action of the natural forces such as winds, currents, waves, tides, etc., with which it was necessary to reckon. It was on these lines that the work at Folkestone had been carried out, and it was thus that work was being carried out, though on a larger scale, with equal success at Dover. Undoubtedly this method of construction necessarily entailed higher costs than had been usual in similar French works until latterly, when the tendency had been towards an improvement; but a large portion of the extra cost must be repaid by the safety and the rapidity with which the works were executed. It would be interesting to know if, in the end, the English practice, which offered at least the marked advantage of speed, was more or less advantageous from the point of view of economy than the French practice. He was inclined to think that it was less; but it was so difficult to make a comparison between works, no matter how similar, that it was impossible to answer the question definitely; and, as the Paper made no mention of the financial aspect, there was Mr. Voisin.

Mr. Voisin. no means of discussing the point further. The statement of the total cost (exclusive of railways, lighthouse-installation, landings, etc.) for sections of pier founded respectively between 6 and 12 feet, between 12 and 18 feet, and below, would perhaps have enabled interesting comparisons to be made with, for instance, the Carnot sea-wall at Boulogne, a structure of wholly different type. Another point for notice was that the action of sea-worms upon timber varied greatly in accordance with local circumstances, which were very ill-defined. Thus at Folkestone the action was very rapid; it was rapid, but less so at Calais; and was relatively slow at Boulogne. One of the points most worthy of study was the question of Portland cement. The finely ground condition of the Portland cement used at Folkestone was evidently one of the reasons for the setting being so rapid as to interfere with the gauging. He would like to point out that for some years it had been thought desirable in French sea-works to return to comparatively coarsely ground cements, as, when mixed with ordinary sand, they were generally less inclined to deteriorate than those which were more finely ground; at all events, the speed of setting had never presented the least inconvenience. It was also surprising that, notwithstanding the steps very wisely taken to aerate the fresh cement, its speed of setting was still so high; but it was difficult to account for this fact, the degree of fineness not being stated. Under the circumstances, the question whether the addition of a suitable quantity of gypsum was good or bad did not arise in connection with the French harbours, and although it seemed to follow from numerous experiments made, notably at the Laboratoire des Ponts et Chaussées at Boulogne, by Mr. Feret, that the addition of less than 1 per cent. of gypsum, either unburnt or burnt, was of little importance as regarded the holding-power of the mortar in the sea, such addition was prohibited. These experiments showed also that unburnt gypsum added in small proportion retarded the setting, and that the addition in large quantity of this same gypsum slightly accelerated it; while the substitution of plaster of Paris (burnt gypsum) in large proportion accelerated it in a marked degree. At all events, however, a mixture of cement with 10 per cent of gypsum, whether burnt or otherwise, was regarded as extremely dangerous to the holding-power of the mortar, not only in sea-water, but also in fresh water. The incorporation with cement of a small percentage of gypsum rendered the boiling-water test less stringent to such an extent that it lost its value. The Author raised the question whether, in the event of the addition of 1 per cent. of gypsum to Portland cement being acknowledged to be without danger, this practice should be regarded as preferable to that of adhering to

cement unmixed with extraneous material, even if the setting was too quick; but he did not answer the question. It followed from the foregoing that in French sea-works no hesitation would be shown in adopting the second practice if the case arose, which, however, as already stated, it did not. An interesting detail to remember was the use, for the purpose of protecting the mortar against the wash of the sea, of a mixture of about equal parts of Portland cement and plaster of Paris. There was no doubt that this mixture solved the difficulty. But, as it might happen that it was only partially possible to remove the protective bed, or even impossible to remove it at all, its use involved serious danger of leaving—if not in the masonry, at least in the facing—mortar which would decompose very rapidly and very completely. This was not the case with the quick-setting cement which was used regularly in French harbours under similar circumstances, and which, notably at the Carnot sea-wall at Boulogne, had been able to resist the action of the sea perfectly during 10 to 15 years. It would be useful to know what was the mean annual accretion over the area dredged. In conclusion, the works described had not only been well designed, but had entirely succeeded in fulfilling their purpose. Mr. Voisin.

Mr. W. H. Wheeler considered that what had occurred at Folkestone in regard to the movement of the shingle along the coast appeared to bear out the correctness of one of the propositions laid down in his Paper on Littoral Drift.[1] It was there contended that the quantity of drift travelling along a coast was limited, and might be cut off entirely, and that its movement could be controlled. This appeared to be borne out by what had occurred at Folkestone. After the west pier of the harbour was constructed in 1856 there ensued a large accumulation of shingle at the back of the wall, due to the drift along the shore, which finally worked round the end of the pier into the harbour. The pier was then extended into 15 feet at low water. The accumulation still continuing, a large area of beach was raised above tide-level, and below this a fine stretch of beach extended westward up to the pavilion. On the raised area of shingle thus accumulated by the harbour-wall, a wide road or drive with pleasure-gardens had been constructed. Further westward, and along the coast past Sandgate and Hythe, groynes had been constructed, and the erosion of the cliffs, which were the Mr. Wheeler.

[1] "Littoral drift: in its relation to the Outfalls of Rivers, and to the Construction and Maintenance of Harbours on Sandy Coasts." Minutes of Proceedings Inst. C.E., vol. cxxv, p. 2.

Mr. Wheeler. source from which the shingle was derived, had been stopped. Consequently, as stated in the Paper, the shingle had ceased to collect, and no apprehension now existed of any further detriment to the harbour from the drift.

The Author. The AUTHOR, in reply, welcomed the particulars given by Mr. Thompson of his tests made with cement adulterated with gypsum, and desired to emphasize the importance of the results obtained. It was made clear that the use of gypsum-adulterated cement on harbour- or other works, where it was probable that concrete-in-mass would be deposited under water, was decidedly dangerous. Cement so treated, if used for concrete which was to set in air, and thereafter to be deposited in water, might—though in the Author's opinion it was by no means certain—be capable of producing sound work; but the adulteration with gypsum of cement to be used in concrete designed to set under water would in all probability lead to disastrous results. The turning of the cement, referred to by Mr. Allen, had all been done by hand without any mechanical contrivance, and no cement had been turned over more than five times unless it was found, either that the increase in bulk more than paid for the cost of turning, or that extra turns were required in order that the boiling-test might be passed. The whole of the testing had been done by one man. The answer to Mr. H. K. Bamber's first question, regarding the increase in bulk of the cement, was given in the Author's reply to the discussion. With regard to the strength of the cement of increased bulk, the Author was unable to say from direct experiment whether, bushel for bushel, the cement of increased bulk would produce work as satisfactory as the original cement; but since the tests made, both of neat cement and of 3-to-1 mortar from the cement of increased bulk, had given better results than tests made from the original cement, the Author considered it safe to infer that the former would produce better work than the original cement. Mr. Brodie's suggestion, that by carrying the foundations to a greater depth into the greensand the apron might have been dispensed with, was not wise. In the first place, the cost of the apron would not have paid for deepening the foundations of the pier by even 6 inches; and in the second place, the foundations, even if carried to a considerably greater depth, would not have been secured against undermining without a protecting apron along the toe of the wall. Mr. Mann also referred to the apron, and advocated the use of a low mound of loose rubble instead of apron blocks. The behaviour of the latter in the event of any scour taking place was exactly the same as that of the rubble described by Mr. Mann. The blocks subsided and cut off

The Author.

further sea-action; whereas with a rubble mound the tendency was for the sea to flatten the slope and scatter the rubble over a large area, and to a considerable distance from the pier. The cylinder foundation at the root of the pier, referred to by Mr. Brodie, was necessary owing to the depth of cover overlying the greensand, and the proximity of the old buttresses and their apron. An open trench similar to that on the widening of the west face, as Mr. Brodie suggested, was out of the question. The inside slope would have undercut the existing old work which was already in a precarious condition. Mr. Clark's suggestion that reinforced-concrete cylinders would have been preferable to the steel cylinders, was made without due regard to the exposed site of this portion of the work, or to the nature of the material to be pierced. The cylinders, if built in one length of reinforced concrete, would have been too large and too heavy for the capabilities of the plant available. If built in situ, the state of affairs after some hours of a rough sea acting on the shuttering and rods might be better imagined than described. The Author thought that the best and most economical method of obtaining a good foundation in similar conditions was to sink steel cylinders by means of compressed air, so that as obstructions were met they could be removed with comparative ease by men who could see what they were doing. In sinking a concrete cylinder, whether reinforced or not, the obstructions had to be dealt with by men encumbered with the diving-dress and groping about in dark, dirty water. The Author fully agreed with Mr. Carron that it would have been preferable if the renewal of the east face of the old pier had been of a permanent character to correspond with the rest of the work; but owing to the necessity for minimizing the break in the line of the pier, it had been quite impossible to construct a wall of sufficient thickness, and piling had been adopted in lieu thereof. The decision to use greenheart piling was arrived at only after many other methods, including reinforced concrete, had been carefully considered and judged unsuitable. The suggestion that a mass concrete wall should have been built between the piles and the old pier did not commend itself to the Author. The space generally was so narrow that the wall would have been no more than a veneer of concrete, of a quality rendered very doubtful owing to the tide rising and falling through its small volume; and the Author failed to see what compensating advantage would have been gained for the extra expense incurred. Mr. Wheeler, referring to the coast past Sandgate and Hythe, assumed that the erosion of the cliffs on one portion of the coast-line produced an accumulation of shingle along that portion. That this was not the case was evident from the fact

The Author. that from Folkestone to Hythe there was no cliff or foreshore of chalk, and that the geological formation was that of the Lower Greensand, from which no shingle was derived. It was not because of the erosion of that portion of the coast being stopped that the shingle was not accumulating at Folkestone at the same rate as hitherto, but because the groyning to the west, which had been greatly increased of late years, had had the effect of trapping the shingle on its way to the east before it reached Folkestone.

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19 November, 1907.

Sir WILLIAM MATTHEWS, K.C.M.G., President, in the Chair.

The discussion on Mr. H. T. Ker's Paper on "The Extension, Widening, and Strengthening of Folkestone Pier" occupied the evening.

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26 November, 1907.

Sir WILLIAM MATTHEWS, K.C.M.G., President, in the Chair.

(*Paper No. 3649.*)

"The Tranmere Bay Development Works."

By Somers Howe Ellis, M. Inst. C.E.

General Description.

The works described in this Paper are situated on the Birkenhead side of the River Mersey, and will eventually occupy 75 acres, of which 62 acres are at present developed. The greater part of this land has been reclaimed by the works from the tidal foreshore of the river, the course of which at this point may be said to be roughly north and south, so that the river side of the works will be described as the east.

The works comprise two graving-docks, respectively 708 feet and 861 feet in effective length, an outer floating basin 15 acres in area, and a shallower inner basin of $2\frac{3}{4}$ acres (Fig. 1, Plate 2). The land to the south and west of the basins is made up with spoil from the excavation, and forms a site for shipbuilding and engineering shops. An area on the south-east is reserved for slipways, and is formed with a slope down to the frontage-wall, there kept low for the purpose of launching vessels over it directly into the river. The works also include two sewer-culverts, made as a continuation of municipal sewers which formerly discharged on to the foreshore.

Simultaneously with the construction of the docks and basins for the Tranmere Bay Development Company, the river-frontage wall and entrances have been built for the Mersey Docks and Harbour Board, who own the land, for the use of the Company during their possession of it.

The geological formation of the site of the works is that of the

Keuper beds of the Upper Trias. Red sandstone rock crops out on the north of the works, attaining its greatest height about the centre of the north side of No. 1 graving-dock, where it is only 3 feet below quay-level. From here it dips south and west, until at about the centre of the south wall of the outer basin it is 70 feet below quay-level. Where not on the surface, the rock is overlaid with 1 foot to 4 feet of hard red sand and detached fragments of rock, locally known as "roach." Above this come beds of sand, gravel, and hard red clay, the latter merging in places into crumbly loam. The whole surface was covered with a layer of mud, generally only about 1 foot thick, but attaining, near the old outfall of the main sewer at the north-west of the site, a thickness of 15 feet, and on the site of the river-wall, towards the south end, as much as 35 feet.

It may be mentioned here that the datum used on these works, as commonly on the Mersey, is the level of Old Dock Sill, 4 feet 8 inches below Ordnance datum. The Old Dock Sill referred to—now non-existent—dates from the earliest days of dock-construction in Liverpool, and is the datum of the Rev. James Pearson's calculations, which were adopted by the Admiralty for the Admiralty Tide Tables.

Reclamation.

The first step, on the commencement of the works in January, 1903, was the reclamation from tidal influence of the site of the docks and basins. This was effected by a system of dams of three types, linked with short lengths of the permanent river-wall. On the north of the works, where the sandstone rock crops out, a length of 250 feet of temporary concrete wall was built, returning at its northernmost end into the face of a section of the permanent wall. Following on the outer end of the concrete dam, a timber coffer-dam, 625 feet in length (Fig. 2, Plate 2), protected the remainder of the frontage-wall, and the entrances of the docks and basin. On the south side a clay embankment 1,800 feet in length, with a pitching of sandstone rubble on the exposed face, extended up the foreshore to high-water mark. The coffer-dam and embankment abutted, at their junction, on to another section of the river-wall, 150 feet in length, which was constructed within a half-tide dam of single sheet-piling.

The concrete dam had, at an average section, a height above the toe of 29 feet, and a width at the base of 18 feet, and it was designed to withstand a head of water of 28 feet. The face had a batter of 1 in 12. The foundations were keyed 9 inches into the rock of the

foreshore, which there slopes towards the river at about 1 in 15. The concrete was composed of 1 part of Portland cement to 7 parts of beach-gravel, with the addition of as many sandstone displacers as could be imbedded in a sufficient matrix of concrete. The site being accessible at low water, construction was carried on by tidal work without difficulty or delay. The wall was pierced with five openings for sluices, each 4 feet square, just above the surface of the foreshore. A gap of 50 feet was also left in the concrete down to 14 feet below the top of the wall, and was utilized for the introduction of a timber framing containing eighteen openings, each 4 feet square, in two rows of nine. These openings, as well as the lower ones, were closed by pitch-pine flaps, hinged at the top and opening outwards. When the dam was in use, the upper flaps were fastened back by chains to upright timbers—accessible from above—so as to avoid leakage under a small head of water. The leakage round them was still at first considerable, but was subsequently much reduced by the addition of continuous rubber piping, fastened round the edge of the bearing face.

The coffer-dam was composed of two parallel rows of whole-timber sheet-piling, 6 feet apart and filled with clay up to the top of the inner row—about 21 feet above datum. The outer row of piles was made good with planking up to a level of 24 feet above datum—about 3 feet above the normal level of equinoctial spring-tides. The rows of piles were tied together with bolts passing through double half-timber walings on each outside. On the land side of the dam, upright timbers, 14 inches by 14 inches, were fixed against the walings at distances of 12 feet, and were nicked and cleated to receive the heads of a series of raking shores in groups of three. The lower ends of the shores bore, just above ground-level, on to a double row of foot-piles, ranged on the river side of the site of the permanent frontage-wall. The thrust of the rakers was transmitted across the site of the wall by horizontal struts on to a second row of foot-piles, which in turn were supported by a bank of tipped rubble.[1] The coffer-dam had in plan a camber of 14 feet in a length of 600 feet, so that the length and inclination of the raking shores varied correspondingly. The shores were stiffened horizontally and vertically, and the foot-piles and horizontal struts were connected by whole timbers parallel to the chord of the dam. As the struts were removed on building the wall, the thrust of the rakers was taken by the concrete in place. A toe-bank of clay was tipped outside the coffer-dam, and was afterwards supplemented by a bank

[1] See *Fig. 10*, p. 155.—Sec. Inst. C.E.

of rubble deposited on the land side to a height sufficient to bury the lowest shores.

The materials for the embankment which formed the southern protection to the reclaimed area were obtained from the excavation by tidal work during the process of reclamation. The height of the embankment varied from 10 feet at one end to 35 feet at the other. The width at the top was 15 feet, and at the bottom it reached a maximum of 140 feet at the lower end, where the landward slope was about 3 to 1, the exposed (pitched) slope being 1 to 1.

Work at the north end of the dam was begun early in 1903, by the erection of a gantry running out from the old quay to a point 400 feet away, where the timber coffer-dam was to start. A stage was formed at this end, and the construction of the concrete dam was commenced from it on the 27th April, while a week later the coffer-dam gantry was started southward from the same point. The piles from this gantry, which ran the whole length of the dam, were driven by an overhanging pile-driver—a trussed timber framing running on three pairs of wheels, with the vertical guides and pulleys on the forward end 14 feet from the last point of support. The boiler, tank, and steam-winch at the back acted as a balance-weight. A pair of piles having been pitched and driven, the pile-driver was run forward 1 foot, or rather more, so that the guides were free from the piles. These were then cut off, cross and longitudinal timbers were erected, rails were spiked to the latter, and the pile-driver was run forward 10 feet or 11 feet to a fresh position. This series of operations occupied 1 day. The staging was subsequently braced across with a pair of diagonal half-timbers, which were bolted to each pair of piles and afterwards removed when the closing sheet-piles were driven for the dam.

After nine pairs of stage-piles, at 12 feet pitch, had been driven in this way, the surface of the rock, which had dipped to a considerable depth, suddenly rose again, until it was covered with only 2 feet of silt and shingle; and for the next 100 feet a different method had to be adopted. The site was accessible at low water of spring-tides, so that it was possible to bare the rock at the position of each pile. Stout wooden boxes were then placed on the surface of the rock, which was excavated inside them to a depth of 3 feet; the piles (unshod) were dropped in, and concreted in place up to the top of the boxes, another 3 feet in height.

A similar method was followed with the sheet-piles of the coffer-dam over this length of 100 feet, where the rock was too near the surface for driven piles to be adequately supported. Southward of

this portion the site was not accessible at any tide, and driving had again to be resorted to. For the next 250 feet not more than 5 feet to 6 feet of silt was found over the rock; but after this it rapidly deepened to a maximum of 10 feet at the south end.

Simultaneously with the construction of the gantry and coffer-dam from the north end, progress was made with a length of the permanent river-wall which was to form an abutment to its southern extremity. A commencement was made in April, 1903, to drive piles for a staging, from a barge moored by the site. The single sheet-piling which enclosed the first section of wall was started on the 5th May and finished on the 26th August, and the first concrete was laid on the 3rd November. During the process of excavation numerous delays were occasioned by blows through the "roach" which overlaid the rock, and which the piles of the dam had in many cases failed to penetrate; but all leaks were stopped by depositing clay outside the piling. The dam itself was caulked by divers until fairly water-tight up to a level of 7 feet above datum. Above this height it was overflowed by the tide; so that the site of the wall had to be pumped out each tide, and work inside to be conducted during two short shifts in the 24 hours. Three 50-foot lengths were constructed in this way during the period of reclamation.

By the end of August the north coffer-dam gantry was connected with the south stage, and from this time onward progress was made with the coffer-dam piles at the rate of about twelve per day. Work was simultaneously carried on in erecting the shores, forming the toe-bank of clay outside the coffer-dam, and filling with clay the 6-foot space between the rows of piles. This clay was not puddled, but was thrown in and levelled by hand; and being subjected to the action of the tide, it formed an exceedingly compact mass.

By the beginning of March, 1904, the reclamation-works were considered sufficiently complete, and advantage was taken of neap-tides on the 11th March, to drive the closing piles in the coffer-dam, and at low tide to shut the sluices in the concrete dam, thus excluding the water from the site of the works. The south embankment was at this time somewhat more backward than the other reclamation-works, and near the point where it joined the abutment at its river end was only up to a level of 13 feet above datum, or 1 foot 5 inches above high water of neap-tides. Clay was tipped here continuously; but settlement of the embankment became more pronounced as weight was rapidly added, and each day the advancing tide was only just excluded. On the 16th March a slight swell, raised by a south-easterly breeze, washed over the top of the embankment, and made a small breach through the crest, which had

been hastily raised with hand-packed clay. Water quickly poured through on to the reclaimed area, until in 2 hours' time, when the water-levels inside and outside were equalized, a length of 50 feet of the embankment had been washed away down to 10 feet below datum—7 feet below high-tide level for that day. The sluice-gates in the dam were at once removed, and the area of the works was again for 4 weeks subject to tidal influence. The greatest head inside the dam as the water was passing out on the ebb was on this date 3 feet 7 inches, and on a subsequent high spring-tide 5 feet. Some anxiety was felt as to the behaviour of the coffer-dam under this reversal of stress, to withstand which there was only the dead weight of the structure, plus the resistance to tension of a number of $\frac{3}{4}$-inch wire bonds, one to every four sets of shores, by which the head of the dam was tied back to the foot-piles. There was, however, no sign of failure, the only effect being an outward movement of the top by about 4 inches—rather more than half the previous inward set due to outside pressure. The breach in the clay embankment was closed by a small double coffer-dam, filled with clay, and backed on each side with a clay slope pitched with rock.

Advantage was also taken of the month's delay to strengthen the coffer-dam, chiefly by forming a bank of rubble immediately behind it, by doubling throughout the outer row of foot-piles and the horizontal struts behind them, and by doubling also some of the raking shores which showed a tendency to twist out of line. These precautions rendered the dam satisfactory and stable. The leakage through it, after the tide was finally excluded on the 8th April, steadily decreased as the clay filling settled. The worst leaks were stopped by outside caulking, those which remained being chiefly at the site of through bolts, and visible only at spring-tides. The greatest inward settlement of the dam, which reached a maximum during the next high springs after reclamation was effected, was about 12 inches from its original line.

Sewers.

Before reclamation of the site could be effected, the two sewers which discharged into open channels on the foreshore had to be dealt with. That on the south side, known as Green Lane sewer, had its outfall at a point about half-way between high- and low-water marks. Before the reclamation-works were far advanced, a concrete manhole was built here, and the flow was diverted through a temporary pipe-line to the south of the site of the works, where it

discharged into a trench cut in the lower foreshore. The new permanent sewer, which was formed of cast-iron pipes, 5 feet 6 inches in diameter, on a bed of concrete, was afterwards constructed within the reclaimed area, and was not utilized in full until after the completion of the works.

The northern sewer, known as Grange Vale sewer, discharged into an open channel at the extreme north-west corner of the works. In order to divert this, it would have been necessary to pump the sewage over or through the dam; and to avoid the inconvenience and cost of this course it was decided to construct the new culvert during the period of reclamation, allowing the sewage in the meantime to flow down the old channel in the foreshore. The culvert is of circular section, 6 feet 6 inches in diameter, and is at a sufficiently low level to pass underneath the inner basin, the invert-level at its outfall being 12 feet below datum, or 3 feet 4 inches below low water of ordinary spring-tides.

A total length of 630 feet was constructed in open cutting by tidal work, and 775 feet in heading through the sandstone rock behind the north wall of No. 1 graving-dock. The portion of the culvert in open cutting was formed of 6-to-1 Portland cement concrete, with 6 inches of an inner lining of 3-to-1 concrete, put in at the same time as the surrounding ring. A minimum thickness of 2 feet 6 inches for the arch was adopted throughout, that of the invert being reduced to 2 feet for a short length at the east end, where the cutting was in rock, and increased to 3 feet 6 inches towards the west, where the culvert was carried on bearing-piles driven into a soft formation. As this sewer was to be taken into use at the time that reclamation of the site was effected, a bursting pressure due to a head of nearly 30 feet of water had to be allowed for on the interior of the arch, with no compensating pressure from above to balance it until water should be admitted to the basin on the completion of the works. The concrete culvert was accordingly fortified with steel hoops, made of old 75-lb. rails bent round and fished at the joint, and embedded in the centre of the concrete ring with a horizontal pitch of 4 feet 6 inches. A longitudinal rail at the top, and another at the bottom, were laid inside the hoops and bolted to them, serving at once to fasten them in position and to fortify the intermediate space. The steelwork was calculated to take the full bursting pressure without relying on the concrete to act otherwise than as a beam between the points of support.

Such a method was preferable, under the circumstances, to any more elaborate and theoretically perfect scheme of reinforcement. The whole of the work was exposed to tidal influence, the trench

having to be pumped out each tide, and a thick deposit of black, foul-smelling mud (due to the neighbourhood of the open sewer) had to be washed and brushed away from the timbering and concrete, before work could be recommenced. Economy of time was therefore more important than a saving of material. Some little difficulty was experienced with the foundations at the west end, owing to an underlying stratum of sand, which was drawn from behind and underneath the timbering by the pumping. The sides of the trench, which was about 28 feet in depth, were formed of half-timber sheet-piles. These, it was found, had not penetrated to the foundation-level of the concrete, and had to be supplemented with 3-inch plank runners driven by hand inside them. The crane- and wagon-roads were carried on piles driven into the foreshore on one side of the trench; and the weight of these, coupled with the loss of ground from behind the runners, forced over the timbering bodily, and at one time the trench threatened to collapse. It was, however, successfully stiffened by diagonal shores between the struts; the culvert was completed without any accident occurring, and proved to be uniformly sound and watertight.

The length of 775 feet in heading was constructed from four shafts, sunk through the sandstone rock above the crown of the sewer, and built round, over the rock, with an encircling concrete rock wall to above high-tide level. Half of the heading was driven from one central shaft, above which an air-compressor was installed to work a pneumatic drill. An average rate of progress of 12 lineal feet per week was maintained at each face by this means, as compared with 8 lineal feet on the shorter lengths driven from the other shafts where hand-drilling was employed.

The culvert in heading was lined with a single 4½-inch ring of wire-cut blue bricks, set in 2-to-1 Portland-cement mortar, and filled solid to the rock with 6-to-1 concrete. For an aggregate length of 60 feet, where soft rock was met with, the arch was lined with two rings of brickwork. Each 12-foot length of lining took 3½ to 4 days to complete, one day being usually occupied in forming the key, which was laid from one end on block laggings.

The culvert was finally completed and utilized on the 9th March, 1904, 9 months after it was begun.

Graving-Docks (Figs. 1 and 3, Plate 2).

The two graving-docks are situated at the extreme north of the site of the works. The quay-space between the coping-lines of the

docks is 44 feet 4 inches, and that between the coping-line of the south dock and the wall of the basin 64 feet 8 inches. The centre-lines of the docks are at an angle of 64° with the line of the river-wall, pointing up-stream.

The main dimensions of the north (No. 1) dock are as follows: length from inner face of sill to head, 708 feet; length of keel-blocks, 660 feet; width at coping-level, 110 feet 2 inches; at sill-level, 80 feet (equal to that of the entrance); and on the floor 71 feet. Level of sill, 12 feet below datum, affording a depth of 30 feet 10 inches at high water of ordinary spring-tides. The level of the floor at the head of the dock is 4 feet below that of the sill, with 1 foot fall from this in the total length, and a fall also of 6 inches from the side of the keel-blocks to the side of an open gutter, which runs round beside the lowest altar. Quay-level, as elsewhere throughout the works, is 25 feet above datum, i.e., 6 feet 2 inches above high water of ordinary spring-tides.

The south (No. 2) dock is 861 feet long from inner face of sill to head. It is 10 feet wider than dock No. 1, namely, 120 feet 2 inches on the coping, 90 feet at sill-level, and 81 feet on the floor, and 3 feet deeper, thus affording a depth of water over the sill of 33 feet 10 inches at high water of springs, and 26 feet 7 inches at high water of ordinary neap-tides.

Both docks have otherwise their main dimensions in common. Each is formed with a torpedo-shaped head, on the vertical walls of which the altars die out as the curve strikes them; and each is provided with four timber-slides, flanked with flights of steps, two piercing each wall. These are inclined at an angle of 45°, and are arched over for a width of 15 feet, so that the coping and top three altars are continuous throughout. The concrete arches over the timber-slides, having a rise of only one-twelfth the span, are reinforced with old steel rails, embedded in the concrete immediately above the crown of the arch. Speaking generally, the principle adopted throughout the works has been to introduce a rough form of reinforcement wherever there was a possibility of concrete being brought into tension, or being seriously weakened by cracks due primarily to contraction.

The chief feature of both docks, so far as their construction is concerned, is that the floors, and, where possible, the altars, are cut and dressed from the red sandstone rock without any facing of masonry or concrete. On the north side of No. 1 dock this rock face extends to a maximum height of 10 feet 6 inches below quay-level, and on the south side of No. 2 dock to a maximum depth of 18 feet 6 inches below quay-level; but along a considerable part of the latter

the concrete wall had to be commenced a little above the floor, and at one end from 10 feet below floor-level, where an old water-worn gully in the rock was encountered. A considerable amount of concrete facing and patchwork, varying in thickness from 18 inches to 6 feet, and well keyed into the rock behind, had to be added to make good defects in the natural face. Great economy, however, was effected by dispensing with a concrete lining throughout; and for the vertical faces of altars or walls, the natural sandstone rock, where sound, could be dressed to a smooth and apparently durable face. For the floor and for the treads of the altars it would have been more satisfactory to provide a facing of some more durable material, owing to the tendency of the inclined beds of sandstone to break away back from the feather-edge formed on encountering a horizontal surface. Many of these beds, on the vertical faces, were marked by thin seams of clay, which were cut out for a depth of 9 inches, and sealed with blue brick in cement.

The concrete walls are 4 feet thick on the coping, battering inwards at the back so as to give a thickness of 7 feet 3 inches at 12 feet below quay-level. Below this point the back of the walls is vertical, the thickness being increased by the 18-inch tread-width of each altar on the face, so that at 40 feet below coping the wall is 16 feet 3 inches thick. In order that the walls may not be dependent on the backing to withstand the thrust of the shores on the upper altars, small counterforts, 4 feet wide, are placed every 20 feet along the walls, and give an effective width of base of 13 feet 6 inches at 14 feet 6 inches below the coping.

Excavation for No. 1 dock was almost entirely in rock. This was got out by hand, except an upper layer about 10 feet deep in the body of the dock, which was removed by a steam-navvy, after being shaken by blasting. The foundations for the walls were prepared by removing all loose or rotten rock, cutting a key-grip 2 feet 6 inches wide and 12 inches deep at the back of the wall, and squaring the face to horizontal and vertical lines. The construction of the walls then proceeded simultaneously with excavation for the body of the dock, the rock altar-faces being cut down by rock-sawyers ahead of the excavators and afterwards redressed to the true line.

The same series of operations was carried on for No. 2 dock, preceded, however, by the removal with a steam-navvy of an upper stratum of soft ground over the site of both the walls and body of the dock. Towards the head, where a depth of 20 feet of silt was found, the walls were constructed in trenches.

The docks were completed by pick-dressing the floor to the true

level, and, as in the case of the walls, by replacing defective rock in it with patches of concrete.

The keel-blocks, which rest on a continuous concrete bed 6 feet 8 inches wide and 15 inches deep, are 2 feet 6 inches apart between centres. They are of cast iron, in four sections, of which the two centre-pieces are wedge-shaped, with planed meeting faces. The total weight of each set is 34 cwt. An elm block, 4 feet by 12 inches by 12 inches, is attached to the top of the iron. Graving-dock No. 2 is provided with a rudder-pit 50 feet long and 14 feet deep, over which the bases of the keel-blocks have a special bridge form.

Pumping-Machinery.

There are two centrifugal pumps 45 inches in diameter, direct-driven by vertical compound engines; and a small independent engine and 14-inch pump are provided for draining the docks. The pumping-station, which also contains an air-compressor for working the penstocks, and dynamos for lighting the pier-heads and graving-dock sides, is situated between the docks, at their river end. Immediately underneath it is the deep-level suction-chamber, into which the suction-pipes of the pumps descend through shafts in the rock, sealed with concrete. The suction-chamber itself, which is 26 feet long, 12 feet wide, and 15 feet high to the soffit of the arched roof, is lined with 12 inches of 4-to-1 concrete, keyed into the rock. Two suction-culverts 7 feet in diameter lead from the chamber to the drainage-wells of the docks, and are controlled by penstocks working in separate shafts. Each culvert is formed of cast-iron pipes for 6 feet on each side of the penstock, the remaining length having only a 12-inch concrete lining to the rock. Both pumps discharge into one delivery-culvert 5 feet 6 inches in diameter, the outlet of which is in the pier-head between the docks. It was specified that the two pumps working together should discharge 1,650,000 cubic feet of water—sufficient to empty the larger dock below mean-tide level—in 2 hours. This test was easily complied with in the official trial.

Entrances.

The entrances to both graving-docks are designed for ordinary ship-caissons, which settle into grooves formed in the walls and sills, and which are towed outside to leave the openings clear. Electric and hand-capstans are provided on the pier-heads, as well as

a system of bollards, which are utilized by means of steam-capstans on the caissons. The walls of the entrances have a batter of 1 in 7. The outer splayed walls are vertical. A stop for a 100-foot caisson is provided outside each entrance in case of repairs being needed, or to increase, if necessary, the effective length of the dock. The caisson-grooves and stops are formed with quoins of Norwegian granite, as are also the outer edges of the sills.

The caissons for the two docks are practically identical in character. That for No. 1 dock, of which a cross section, showing the arrangement of pipes and valves, is given in Fig. 5, Plate 2, is divided below the main deck into four water-tight compartments. The main central portion, 63 feet in length, is divided into two by a longitudinal bulkhead; the end compartments, formed by cross bulkheads, extend the full breadth of the caisson, the longitudinal division being here pierced so as to allow of free passage. A 15-inch centrifugal pump, driven directly by a vertical compound engine, is installed on the main deck, and by means of this, and of the inlet- and outlet-valves, each compartment can be filled or emptied separately, thus ensuring complete control. The caisson can be lightened so as to rise sufficiently to clear the grooves in 15 minutes after pumping is commenced. It can be handled in 10 feet of water, and can be floated so as to clear the grooves when there is 23 feet of water over the sill. It is designed to withstand a pressure due to a head of water of its own height (38 feet above the sill) on either side. The keel and stem-plates are faced with greenheart, so as to give a total width of 22½ inches, the grooves being 23 inches wide. Rubbers of American elm are fixed on each side of the caisson at the point of greatest width. Concrete ballast weighing 148 tons is provided, and the inside of the hull, up to the turn of the bilge, is cemented over so as to cover the rivet-heads. The design of the caisson for No. 2 dock differs slightly, to suit the greater length and depth.

Filling-culverts, of rectangular section, 6 feet in width, are formed in the walls on each side of the entrances, and serve also, when used to empty the docks down to the level of the falling tide, to sluice the outer platforms of the entrances. They are controlled by double sets of greenheart sluice-valves of a tapered form, which fit into granite-faced cloughs, and which are raised and lowered by hand-power with screw-gearing.

Basins.

The outer floating basin is entered directly from the river by two entrances—one 91 feet 6 inches in width and the other 30 feet—each being closed with a single pair of greenheart gates. The sill of the main entrance is 15 feet below datum, giving 33 feet 10 inches of water at high water of ordinary spring-tides, and 26 feet 7 inches at high water of neap-tides. The small entrance has its sill 12 feet 6 inches higher, i.e., 2 feet 6 inches below datum. The basin itself is excavated to 20 feet below datum, thus giving a depth of water of more than 30 feet in which vessels can lie throughout neap-tides, assuming that the gates are opened for an hour at high-water only.

The wall which forms a frontage to the river between and on each side of the entrances is double, a quay-space of 30 feet being provided between the coping-lines of the faces (Figs. 4, Plate 2). The two component walls are connected by cross walls, 20 feet apart and 4 feet thick, the spaces between which are filled with dry rubble over a 6-foot bed of concrete forming a base to the whole structure, which is founded on rock throughout.

The other three walls (Figs. 4) form quays along which vessels can lie for repairs and fitting-out, and are respectively 1,000 feet, 850 feet, and 990 feet in length. When the two latter walls, which lie on the west and north sides of the basin, were commenced, it was intended to excavate the basin to a depth of only 14 feet below Old Dock sill, i.e., 6 feet above the level finally adopted; and the foundations of these walls were, in consequence, only carried down to a mean level of 18 feet 9 inches below datum. The north wall is founded on rock throughout; so that the only precaution considered necessary was to leave a slope of rock at 1 to 1 immediately in front of the toe of the wall. The concrete base of the wall is keyed into the rock by a grip, 4 feet wide and 1 foot deep, cut into the back of the foundation; and although for the most part unsupported in front, the wall may be considered secure against any tendency to move forward. The west wall is founded on hard red clay, except for a length of 200 feet, which is on sand. Bearing-piles in threes, spaced 7 feet apart longitudinally, were driven down to the rock throughout its length, and project upwards 18 inches into the concrete, which serves to connect them rigidly, no walings or ties being employed. For a length of 750 feet, over which the foundation of the wall is (for the reason above-mentioned) higher than the final level of the bottom of the basin, a benching of

ground, 23 feet wide and sloping down at 3 to 1 beyond this, was left 6 feet above the rest of the floor, as a support to the toe of the wall. As an additional precaution, a row of half-timber sheet-piles was driven in front of the toe along the 200-foot length where the wall is founded on sand, in order to prevent any tendency for ground to creep away from under the wall. All piles used here, as elsewhere in the basins, are of American pitch-pine.

The south wall, 1,000 feet in length, was constructed having in view the excavation of the part of the basin alongside it to the depth —20 feet below datum—which was afterwards adopted over the whole area. The foundations were accordingly carried down to a mean depth of 24 feet 9 inches below datum; and the wall was strengthened so as to give it a thickness of 18 feet 9 inches at the top of the toe, 43 feet below the coping. For a length of 200 feet at the river end this wall is founded on rock, for the next 200 feet on clay, and for another 100 feet on firm gravel. At a position which came on the last-named stratum three heavy counterforts were added, to carry the piers of a 150-ton stationary crane. The remaining 500 feet of the wall rests on a bed of fine sand, generally firm, but charged with tidal water. The wall is supported on three rows of piles, the front row being pitched 5 feet apart between centres, the two back rows 6 feet 8 inches apart. So far as could be seen, by comparison with borings, the piles throughout were driven down to the rock, which reached its lowest point—about 21 feet below the formation of the wall—midway along this length. The top 14 feet of backing—mixed clay and rock, rammed in layers—was filled in behind this part of the wall in October, 1905, at a time when the last portion of ground had just been excavated from in front, and when also some early frosts had caused the appearance of vertical hair-cracks at intervals in the concrete face. As the backing was brought up to quay-level, a wave of forward movement passed along the wall, attaining, at the centre of this 500-foot length, a maximum of 3 inches in a week. A series of observations made each week after this showed an irregular but continual forward movement of the wall, averaging about $\frac{1}{4}$ inch per week, until water was finally held up to high-tide level in the basin, by the stepping and closing of the gates, in July, 1906. By this time the centre of the curve formed by the bulged portion was 11 inches out of the true line of the wall. Water was admitted to the basin in the middle of April, so that for 3 months the tide ebbed and flowed in its area, and no doubt favoured the conditions causing movement of the wall. As, however, no subsidence or tilting of the wall was observable, it was not considered necessary to take any measures for anchoring or

otherwise supporting it. Some slight movement was observed during this 3 months in every quay-wall on the works which was not founded upon rock; but the movement was entirely one of sliding, or more probably a forward creep of the ground itself under the walls. In all cases except the one mentioned it was insignificant in extent.

Excavation for the basin-walls was carried down in trenches for the bottom 20 to 25 feet, after any ground above this level had been removed. The only difficulty with the foundations occurred in the south and part of the west wall, to which tidal water had access through beds of sand and gravel. The trenches were drained with 6-inch and 9-inch earthenware pipes, laid in a grip in the bottom, and leading to a sump either ahead of the length in process of excavation, or kept open in the concrete behind. The bottom of the trench, where at all soft or wet, was paved with squared blocks of sandstone procured from old masonry walls on the site and weighing ½ ton to 2 tons. These were kept a foot away from each other and from the sides of the trench, so that concrete could be worked in between, but were placed directly on the ground. Springs which developed in the bottom were dealt with by confining the flow to a vertical pipe, until the head of water overcame the upward pressure, and then running in cement. It was usually found advisable to keep a hand-pump at work, so as to reduce the head of water in the pipe until the concrete round it had set, any tendency for water to burst up through the green concrete outside the pipe being thus obviated. The basin-walls are provided with 4-inch weep-holes at every 50 feet, at a height of 9 feet above dock-bottom. Before the tide was admitted to the basin, those in the north wall were stopped with wooden plugs, in order to prevent water from getting to the back of the graving-dock wall, in which there are no weep-holes.

Excavation for the outer basin was commenced during the period of reclamation, in order to provide material for the clay dam, with two 5-ton cranes furnished with grabs. These worked for about 6 hours at each low tide, the amount excavated by each averaging 600 cubic yards per week. After the site was reclaimed in April, 1904, two, and subsequently three, steam-navvies were installed; and the full amount of excavation, comprising about 80,000 cubic yards of rock and 520,000 cubic yards of clay and sand, was got out by the end of January, 1906. The average amount removed per week by each navvy, working by day only, was 2,700 cubic yards of soft material, or 1,700 cubic yards of rock, the latter being shattered by

blasting ahead of the working-face, with a consumption of about 27 lbs. of cheddite for each 100 cubic yards excavated. The maximum total output in one week was about 3,000 cubic yards of rock and 9,000 cubic yards of soft material, when two of the navvies were working day and night. The bulk of the spoil was hauled up an incline with a gradient of 1 in 35. A second incline, alongside the west wall, supplemented and afterwards superseded the first, and was finally cut away by hand, the spoil from it being drawn up by three cranes stationed on the wall above. The whole of the material from the excavation was utilized in making up ground for the shipyard and quays.

The inner basin is intended for barges and light-draught vessels only, and is excavated to 2 feet below datum, giving a depth of water of 13 feet 7 inches at high water of ordinary neap-tides. It is connected with the outer basin by an open entrance 40 feet in width, crossed by a swing-bridge. A low concrete retaining-wall maintains the difference in level between the sill of this entrance and the floor of the outer basin, and is backed by a concrete apron, 3 feet thick, extending to where grooves in the walls of the entrance afford provision for a temporary stank across it. The north wall of the inner basin is an old one of masonry, which originally formed the limit of the foreshore. The other walls, which are new, are practically similar in dimensions and construction to those of the outer basin, but are on a smaller scale. The north end of the east wall is founded on rock; the remaining 200 feet is borne on piles, and has a continuous row of half-timber sheet-piles driven in front of the toe down to rock, in order to prevent percolation of water from the basin to the back of the graving-dock walls. The south wall is on bearing-piles throughout.

Launchways (Figs. 4, Plate 2).

The river-wall towards its south end is brought up only to 11 feet above datum (7 feet 10 inches below high water of ordinary spring-tides) and forms a frontage to the launchways, which rise up behind it at an inclination of $\frac{5}{8}$ inch per foot. Throughout the greater part of its length the rock was found to be at too low a level (34 to 42 feet below low water of ordinary spring-tides) for the foundations to be carried down to it within timber dams, as was done in the case of the frontage-wall immediately south of the basin. As the stratum overlying the rock—blue

river-deposited silt 30 to 35 feet in depth—was not considered sufficiently stable to afford a foundation for a wall resting on bearing-piles, recourse was had to a series of monolithic concrete piers. These are 15 feet thick and 26 feet from front to back, and are spaced with 25-foot clear openings between them. Above 7 feet below datum (1 foot 8 inches above low water of ordinary spring-tides) the wall is built solid throughout. The portions over the bays were formed on a mass of rock rubble, the silt having been previously dredged out beneath so as to give a minimum depth of 10 feet 6 inches of rock filling. A continuous row of greenheart sheet-piles was driven across the front of each bay down to rock, battering back at the head, and held there by a waling which was 18 inches behind the face of the wall above. The pile-heads are caught also by the concrete of the wall.

The method first employed for forming these piers was to sink rectangular wrought-iron caissons, 26 feet by 15 feet, down to the rock by weighting, grabbing from the centre, and bolting on fresh sections above as the cutting edge was lowered. When finally stopped on the solid stratum, these were pumped dry, excavation by hand was employed to bare and key the rock foundation, and the caisson was filled with concrete up to the required level, the upper sections (between low- and high-water mark) being then unbolted and removed. Progress by this method was slow, owing principally to the difficulty in getting sufficient weight on to the caissons to overcome the high skin-friction offered by the silt. The expedient was then adopted of lining the caissons in place with 4 feet of concrete, stiffened with a cross wall in the centre, the slope of the cutting edge being continued inwards until the full thickness of lining was reached. Better results were thus attained, and in the last piers (eight in number out of a total of eighteen) the design was simplified by omitting the wrought-iron caissons above the bottom section, and by forming with wooden moulds, as the cutting edge was lowered, a hollow concrete monolith, with two central spaces in which the silt was grabbed out. When sunk and pumped out, these were filled solid with concrete as before. The progress by this means was strikingly better than with the former methods, a monolith being sunk as much as 10 feet in a week, when the walls had been brought up well in advance, or an average of 3 feet 6 inches when the operations were carried on alternately for each lift of concrete.

The launchways are formed of rock rubble, tipped on to the tidal foreshore. For the top 10 feet this was deposited in

layers 2 feet thick, and levelled by hand, with a layer of 1 foot of gravel between each layer of rock.

For the first slipway to be used, on the north of the area in question, a system of piled foundations has been adopted. A concrete platform, 40 feet wide and 5 feet thick, supported by three double rows of bearing-piles, under the lines of the keel-blocks and groundways, is laid for a length of 400 feet up from the river-wall. Above this the piled foundation runs under the keel-blocks alone. For a length of 50 feet, in the region where a highly-concentrated load is momentarily brought on to the ways during launching (when the stern of the vessel is first water-borne) the bearing-piles under the groundways are doubled in number.

Concrete.

The concrete in the body of the walls is composed of 8 parts (by measure) of beach-gravel to 1 part of Portland cement; and a facing of 4 to 1 concrete, 6 inches thick, was laid at the same time. The Portland cement, which was obtained from the Medway, was subjected to the usual tests for tensile strength, soundness, and fineness, and was stored on the site of the works for 3 weeks before being used. It was not shot or turned, except in the case of a few cargoes which had to be used before the specified time of storage was completed.

The bulk of the gravel was obtained from Piel Island, near Barrow-in-Furness, and was composed of granitic and syenitic pebbles of different sizes, with clean angular sand in about the correct proportions for rendering it a component of concrete. The cargoes varied somewhat, and, to ensure approximately correct proportions being used, a store of Fleetwood gravel, in which the percentage of sand is higher, was kept on the site, to be mixed on occasion with the ordinary supply of Piel gravel. Pure sand, obtained from the excavation, was also used for the same purpose, while a pile of the larger pebbles, picked out and broken by hand, was kept at each stage to mix with gravel in which the proportion of sand was too high.

Ten representative samples gave the following proportions of sand and shingle, when screened through a sieve of ⅛-inch mesh; quantity measured, 1, sand 0·33, shingle 0·79, representing a proportion of sand to shingle of 1 to 2·4, and a mixture in 8-to-1 concrete of 1 part cement, 2·64 parts sand, and 6·32

parts shingle. These proportions may be taken as fairly representative of the concrete used throughout the works. Owing, however, to the shrinkage in bulk of the concrete when in situ (amounting on an average to 5 per cent. of the gravel as gauged), the actual quantity of cement used worked out at almost exactly one-eighth (by measure) of the concrete in place, leaving out the 4-to-1 facing. Taking the walls as a whole, an average of 3 cwt. of cement was used for each cubic yard of concrete.

Six tests were also made to determine the percentage of interstices in the sand (when screened through a sieve of $\frac{1}{8}$-inch mesh) and in the remaining shingle. The average of these gave a percentage of interstices in the sand of 30·1, or a ratio of 1 to 3·32, and in the shingle a percentage of 32·8, or a ratio of 1 to 3·05. These results, when compared with those of the former tests, tend to show that the cement used was sufficient to fill the interstices of the sand, and that the mortar thus formed was sufficient to provide a matrix for the stone, the margin, however, being too slight to ensure such absolutely water-tight concrete as might be necessary for other classes of work. Practice shows it to be enough for dock-walls, with the exercise of ordinary care in the manipulation. The weight of the gravel used varied from 125½ lbs. to 133 lbs., and averaged 129 lbs., per cubic foot. The weight of the concrete varied from 145 lbs. to 151 lbs., and averaged 148 lbs., per cubic foot when dry, and 150 lbs. after a month's immersion in water.

The larger part of the concrete was mixed by passing through gravity mixers, after being gauged and turned over twice dry on high-level stages. It was conveyed in Decauville skips or by cranes to the site of the work in progress, where four men in each gang were employed to shovel it into place on the wall and level it. Rammers were not generally used. The concrete was mixed sufficiently wet to settle into a compact, plastic mass when shovelled and trampled into place, and was further solidified by the insertion of sandstone displacers, embedded in the concrete at each foot in height. These displacers were kept at a minimum distance of 9 inches from each other horizontally, and varied in size up to stones of 1 ton or more near the bases of the walls. Their percentage decreased with the thickness of the walls from 10 per cent. to 7 per cent. of the whole mass. Throughout the greater part of the graving-dock walls, hand-mixing and barrow-work were necessitated, and only about 5 per cent. of sandstone displacers were used, chiefly in the form of bonders between the layers of concrete.

Equipment.

The equipment of the docks and basins accords with their intended use—exclusively that of fitting-out and repairing. A 40-ton electric crane travels along the quay-space on the south side of No. 2 dock, and serves that dock and the north side of the outer basin. A 150-ton electric crane is installed on the south wall of the outer basin, adjacent to the main engine-shops. This crane has a derrick-jib, and revolves on a fixed platform, which is supported on a central pivot-pier and three stay-piers—the whole structure being of steel lattice-work. Tracks for 10-ton steam-cranes run along the north side of No. 2 dock, along both sides of No. 1 dock, and on the quays of the inner basin.

Three 10-ton electric capstans are provided for the graving-docks —one at the head of each, and one on the pier-head between them. Two 5-ton capstans stand on the outer pier-heads, and two 3-ton capstans, also worked by electricity, midway along the sides of the docks. Hand-capstans are also provided on each pier-head. Mooring-posts are fixed at intervals of 80 feet along all the walls.

Electricity for working the cranes, capstans, and gate-opening machinery, for lighting the shipyard, and for furnishing motive power in the engineering shops, is supplied from a generating-station at the south-west corner of the site. A gas-producing plant is here installed, having, at present, three producers, with their attendant washing-, scrubbing- and drying-machinery. These feed five gas-engines, of 1,550 HP. in all, linked with dynamos generating 860 kilowatts. Space is left for accommodating three more 400-HP. gas-engines and dynamos. An air-compressing plant and hydraulic pumps and accumulators are also provided. The main power-house is a brick building, 275 feet long and 38 feet wide inside, roofed with glass, and furnished with an overhead travelling crane. The concrete foundations for the engine-beds and walls are carried down through made ground to the natural clay, 12 feet below floor-level.

Shop-Foundations.

The extensive engineering shops which are in course of construction on the land to the south and west of the basins present no noteworthy feature in their design. They are almost entirely of steel framework, roofed with glass and corrugated sheeting, and

having their sides of the latter material. The buildings being all on made ground, special provision had to be made for the column-foundations of those shops in which heavy overhead cranes were to travel. In the main erecting- and machine-shop, which may be taken as an example, and which is 1,035 feet long, with overhead cranes, 60 feet above floor-level, travelling along the centre portion of 74 feet span, the concrete blocks supporting the column-bases were carried on groups of "Simplex" concrete piles, varying in number from four to twelve for each column.

The Simplex system of concrete piling consists in driving a hollow steel tube to a firm bearing, and ramming down concrete from above as the tube is gradually withdrawn (Figs. 6, Plate 2). The tube or "form" used was 40 feet in length, 16 inches in outside diameter, and of ½-inch metal. As obtained from Germany, the forms were composed of three lengths of tube, welded together. The only pattern obtainable in England had one joint, spliced with a riveted cover-plate, and was found not to be so satisfactory. To the bottom of the form is attached what is known as the "alligator" point. This is made of annealed cast steel, and consists of a sleeve, 17 inches in outside diameter, riveted to the form, and having a pair of toothed jaws hinged to it in such a manner that when closed they form a flat, spear-shaped point to penetrate the ground. When the form is drawn up a foot or more, the jaws open and allow the concrete to pass freely through, and to fill the space occupied by the form.

The apparatus used consists of a heavy timber frame, with upright guides, stayed in position to hold the form, and horizontal beams on which stand the winch and boiler. The form is held in position by a cast-steel driving-cap, grooved to slide between the leaders, and having on the underside a ring which fits inside the form. A plug of elm or other hard wood is inserted into the top of the cap, standing up about 6 inches, and receives the blows of the hammer, a 30-cwt. iron block, falling 6 to 8 feet. On these works the form was driven to a final set of 1 inch in four blows with a drop of 6 feet, penetrating on an average 20 feet of close made ground and 10 feet of the natural stratum of sand or clay. The made ground contained a number of large fragments of sandstone rock—up to 3 tons in weight—which were penetrated by the form without any marked stoppage or divergence from the vertical.

No difficulty was found in drawing the form. The pulling tackle consists of a set of five-fold blocks, reeved with a steel-wire bond. The winch and tackle were capable of exerting a pull

of about 50 tons, but were rarely worked up to anything like the full load.

The concrete (6-to-1) is hoisted up and tipped into the top of the form in special buckets with hinged bottoms, each bucket holding the equivalent of 2 feet in length of the finished pile. After the first bucket is deposited, the form is drawn 1 foot, and the concrete is well rammed with a plummet weighing about 2½ cwt. After each succeeding bucketful the form is drawn 2 feet, and the concrete rammed as before, thus ensuring a head of 1 foot of concrete inside the form. It was found advisable to increase this head to 2 or 3 feet near the surface of the ground.

In firm made ground of good material, no drawback is apparent to the use of this system of concrete piling. A concrete monolith is formed to the full outside diameter of the form, and, owing to the high skin-friction, and the effectual consolidation of the ground round the piles, the load that can safely be imposed on them would seem to be limited by the crushing strength of the concrete. In this case 20 tons per square foot of pile-head was taken as the maximum load. With piles pitched 3 feet between centres, as was here the practice, one pile, even when newly driven, does not appear to be at all disturbed by the driving of the adjacent piles. A limitation to the system, however, was clearly shown on one part of the site, where a layer of spongy blue clay, 6 feet to 10 feet in thickness, had recently been deposited on the surface. The concrete piles, when bared, were here found to be deformed and reduced in diameter (in some cases to as little as 11 inches) by the pressure of the machine on the elastic ground, and by the driving of adjacent piles. The excavation for the concrete blocks had to be taken down to the bed of good filling which underlay the blue clay, where the piles were found to regain their normal character.

Owing to the larger number of concrete piles which it was thought advisable to employ, and to the more expensive plant necessitated, the cost of this system worked out at about 15 per cent. more than that of timber piles, with the advantage that the ground was more thoroughly consolidated and a more permanent foundation was secured. As regards labour and materials only, the concrete piles are individually cheaper than pitch-pine, averaging about 1*s*. 8*d*. per lineal foot. The number of piles driven per working-day with each machine—including moving it from one foundation to the next—was, under favourable circumstances, nine, with a mean penetration of 30 feet.

Dredging.

In order to provide deep-water access to the entrances of the docks and basin, about 64,000 cubic yards of material—nearly half of which was sandstone rock—had to be dredged away from the river-bed. The surface of the rock towards the north end was bare at low water, and the greater part was removed with bucket-dredgers at high tide, after being drilled and blasted when the surface was dry. The soft material was also got out with bucket-dredgers.

The layer of rock at a lower level was shattered with a Lobnitz rock-cutter, having a 12-ton cutter, worked with a drop of about 8 feet. The procedure with this was to jump a series of holes, 3 feet deep and 3 feet apart, in rows 18 inches apart, each hole thus representing ½ cubic yard of rock. About five blows were needed for each hole. An average of 7·14 cubic yards of rock per working-hour was broken by this means, and so effectually pulverized as to be easily removed by a small bucket-dredger which had some difficulty in dealing with the rock as shaken by blasting. The cost of labour, maintenance, and coal, for breaking only, came to 1*s.* 6*d.* per cubic yard.

The only difficulties attendant on the use of the rock-cutter were (1) that of manœuvring the barge on which it was mounted, in the strong tide-way, so as to space the holes accurately; and (2) that of preventing kinking and ultimate fracture of the 1⅝-inch wire-rope by which the cutter was lifted. The only effectual precaution against the latter seemed to be the exercise of great care on the part of the driver in dropping the cutter, to allow only sufficient slack, to permit it to reach freely to the end of its stroke.

Completion of Works.

Water was admitted to the docks and basins on the 9th April, 1906, almost exactly 2 years after reclamation of the site was effected. The graving-dock entrance-caissons, which were built in the docks, were floated into their grooves as the water rose. The gates for the basin-entrances were brought from Liverpool by a floating crane as soon as the dam was sufficiently removed to allow a free passage through it, and were all stepped by the 25th June. They were closed so as to hold up the water in the basins to high-tide level on the 19th July.

The Contractor for the works was Mr. L. P. Nott, represented on

the ground by Mr. Robert Brodie, M. Inst. C.E., who, with the Agent, Mr. G. H. Dutton, and the Contractor's Engineer, Mr. A. C. Wilson, Assoc. M. Inst. C.E., was responsible for the design and execution of the coffer-dam.

The Engineer for the river-frontage wall and entrances, which have been constructed for the Mersey Docks and Harbour Board, was Mr. Anthony G. Lyster, M. Inst. C.E., to whose courtesy the Author owes permission to mention certain of the Board's works in this Paper.

The Engineers for the Tranmere Bay Development Company's works were Messrs. J. T. Wood and A. F. Fowler, MM. Inst. C.E. and the Author acted as Resident Engineer.

The Paper is accompanied by eight drawings, from which the illustrations in Plate 2 have been selected for reproduction.

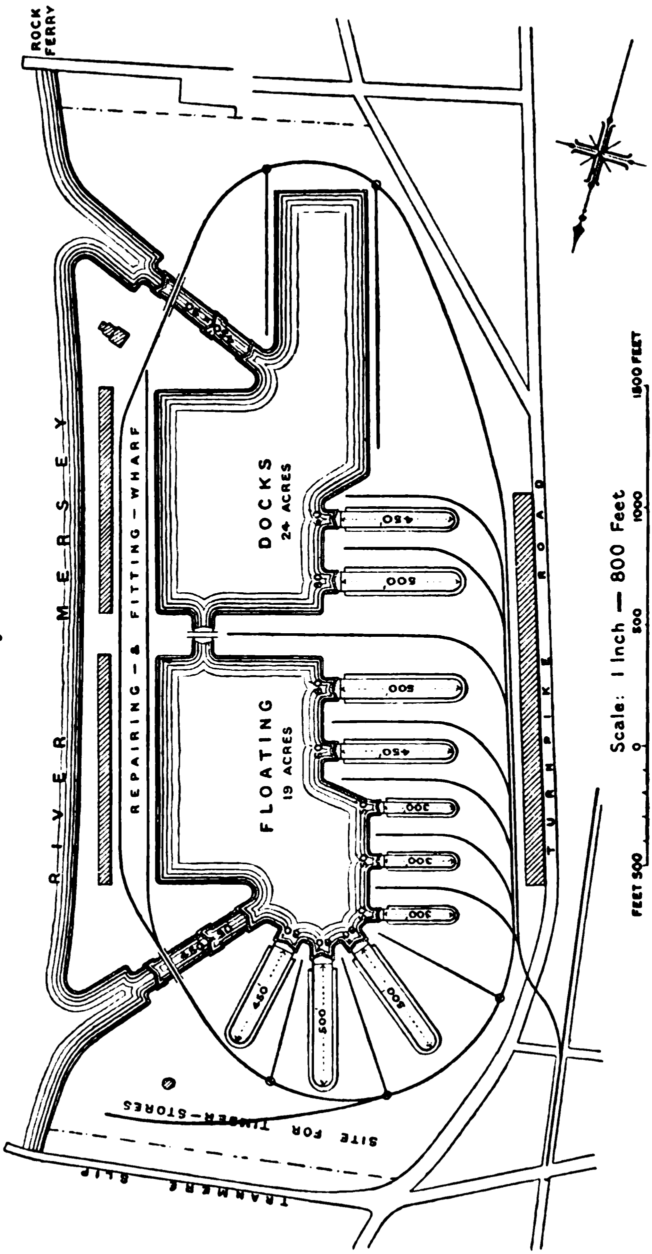
Discussion.

The PRESIDENT, in proposing a vote of thanks to the Author for his Paper, which described a very successful commercial work well carried out, mentioned that the Author was in China, but Mr. John T. Wood, one of the engineers of the work, was present and was prepared to afford any further information required by members in regard to the work. The President.

Mr. JOHN T. WOOD observed that since the writing of the Paper some modifications had been made, although not of a very important character. When the Paper was written the Tranmere Bay Development Company had 60 acres of the foreshore available for works; since then another 15 acres, extending right up to the southern boundary, had been added; that was, they had exercised their option of taking the whole of the land. The extra 15 acres gave an additional frontage of 300 feet to the river, and slipways were now in process of construction. There had also been an alteration in the inclination of the launchways, which were originally designed to have a slope towards the river of $\frac{5}{8}$ inch per foot. The north slipway, nearest the outer basin, had been constructed in that way, but it had been thought desirable in all the others to construct the first 50 feet from the river-wall with $\frac{5}{8}$ inch per foot slope, making the remainder of the slope $\frac{1}{2}$ inch per foot. The Author drew attention to the forward movement of the south wall of the outer basin, at a point between the 150-ton crane and the south-west corner. During the last 4 months there had not been the slightest alteration in that wall; and $11\frac{3}{4}$ inches was the total advance. It was not expected that the wall would move again; it was thought now to have settled into its natural position. In slipway No. 1 there were 450 piles covered by a blanket of concrete 4 feet 6 inches in depth and 44 feet in width, the concrete being reinforced with steel rails. He had placed on the wall two diagrams (*Figs. 7* and *8*) of a scheme which had considerably interested him; they were designs made by the late Mr. James Abernethy in 1862, for the utilization of the same site for docks. The Tranmere Bay Company occupied only 75 acres of the shore available between Birkenhead and Rockferry, but Mr. Abernethy planned the utilization of the whole area of 175 acres. To students of the advance that had been Mr. Wood.

Mr. Wood.

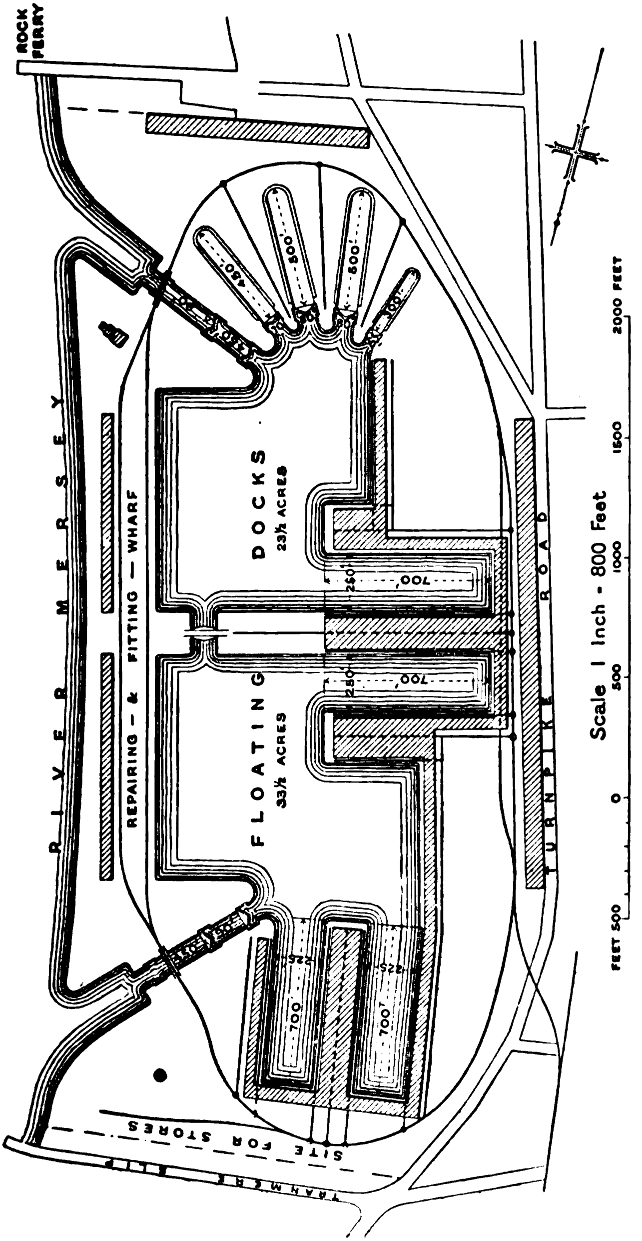
Fig. 7.



Mr. James Abernethy's Plan for Docks for Warships at Tranmere Bay, 1862.

Mr. Wood.

Fig. 8.



Mr. James Abernethy's Plan for Commercial Docks at Tranmere Bay, 1862.

Mr. Wood.

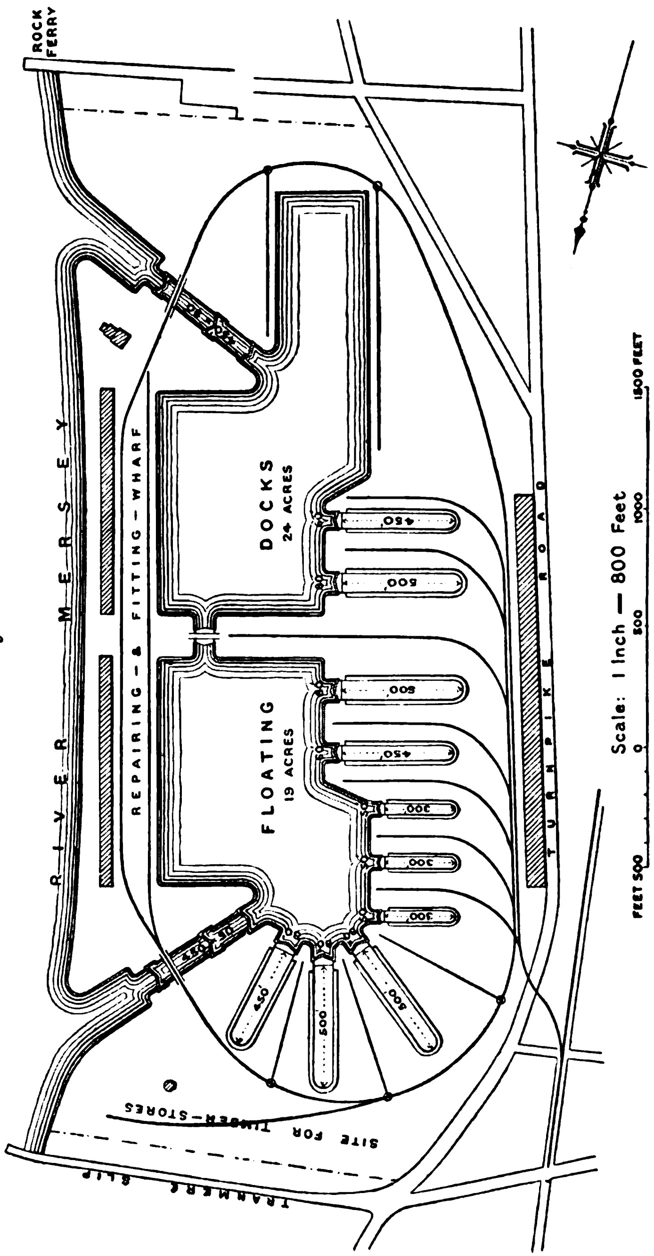
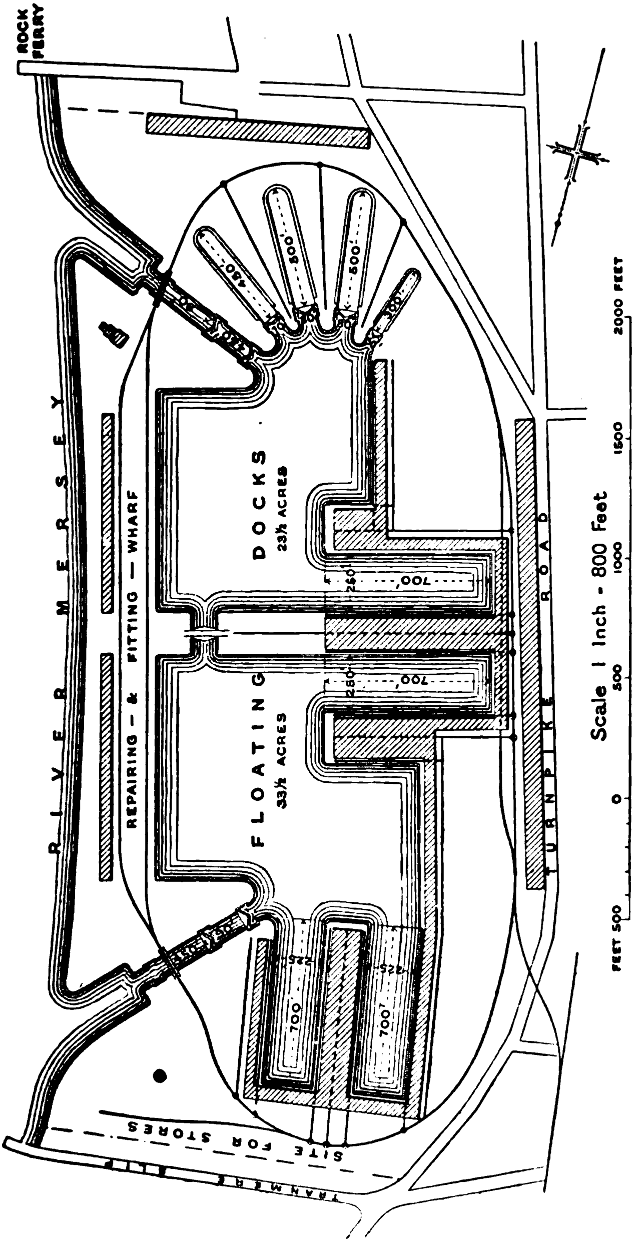


Fig. 7.

MR. JAMES ABERNETHY'S PLAN FOR DOCKS FOR WARSHIPS AT TRANMERE BAY, 1862.

Mr. Wood.

Fig. 8.



MR. JAMES ABERNETHY'S PLAN FOR COMMERCIAL DOCKS AT TRANMERE BAY, 1862.

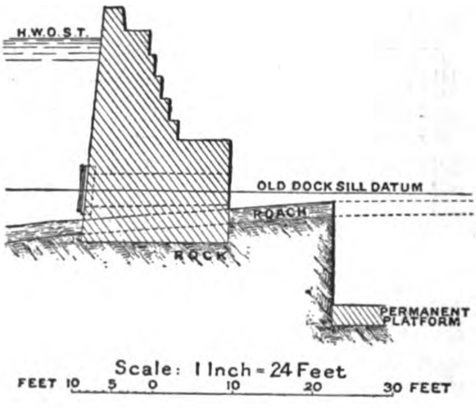
Mr. Wood. made in the building of ships, both for war and for commercial purposes, the diagrams were of considerable interest as showing what were considered 45 years ago to be up-to-date high-class works. *Fig. 7* showed the intended provision for warships and other large vessels. There were to be ten graving-docks, the largest of which were 500 feet long, with entrances 80 feet in width, and the smallest 300 feet long with 50-foot entrances. There was to be ample accommodation in the wet docks, which were to be approached by entrances having locks 450 feet in length. There were to be quays between the graving-docks, all the quays being supplied with railways for getting material to and from the vessels. As a matter of fact, an arrangement of that character, although designed in 1862, would put some of the docks in Liverpool, now held by the Mersey Docks and Harbour Board, very much in the shade as regarded railway-facilities and accommodation.

Sir Wm. White. Sir William White asked what was the intended depth of water.

Mr. Wood. Mr. Wood could not say, as he had not seen any sections.

Mr. Brodie. Mr. Robert Brodie wished briefly to supplement the information given by the Author with regard to the dam. The concrete wall adopted at the north end (*Fig. 9*), where the foundation was on rock above low-water level, was probably more expensive than a timber

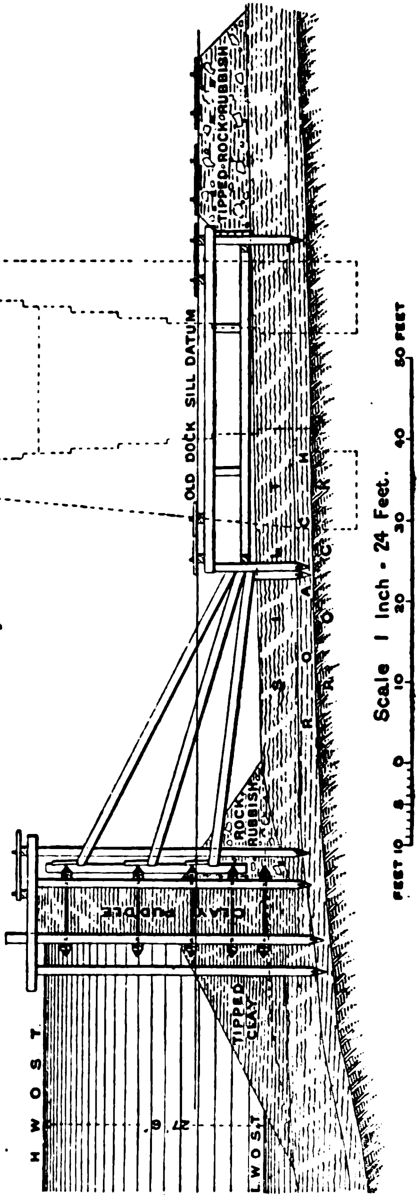
Fig. 9.



dam would have been, not only in construction, but also in removal, after which the materials were of no value; but the advantages of adopting this construction were, first, greater stability at the most exposed part of the dam, secondly, no struts were required and therefore the dam could be kept closer to the wall, and thirdly, and

chiefly, the necessary sluices could be much more satisfactorily **Mr. Brodie.** provided through concrete than through timber and clay puddle. One of the lower sluices with the timber flap was shown in *Fig. 9.* The construction of the strutting of the timber and puddle portion of the dam had been carried out under considerable difficulty, as the whole of the foundation was either at or below low-water level, and was in silt of a very objectionable character. Before commencing to deposit the clay between the sheet-piles of the dam, he attempted to remove that silt by pumping, but it came in almost as quickly as it was cleared out: he decided therefore to trust to the weight of the clay forcing the bulk of it out, and accordingly commenced filling from both ends of the dam and allowed the toe of the slope to keep in advance of the closing-piles, so that the silt might escape. As the dam was filled and the closing-piles were driven, clay was deposited on the outside of the dam, and that no doubt settled well down into the silt and prevented what was a real danger, namely, the bursting through of

Fig. 10.



Mr. Brodie. the water between the points of the piles and the rock, as the piles penetrated the "roach," but could not as a rule reach the solid rock. Rock rubbish was also deposited against the inner side of the dam (*Fig. 10*). The short piles which took the thrust of the rakers were also driven in the silt and had therefore no practical effect, so that it was necessary to transmit the thrust by struts to another row of similar piles backed by a rubble bank, which besides supporting those piles served as a foundation for two roads which were utilized for excavating the foundation of the permanent wall. Those roads were connected by a road inclined at about 1 in 30, running up the inner slope of the southern embankment shown in Fig. 2, Plate 2. The inner white strip there shown should have come down to the rubble bank; the outer white strip was the top of the bank, on which the roads were laid at about coping-level, and continued over the top of the dam. They were used for concreting the permanent wall and for unloading cargoes of gravel, cement, granite, etc. The cost of the coffer-dam had been approximately £12 per foot for the concrete portion, and £24 per foot for the timber and puddle portion, including erection, maintenance, and removal, and crediting the value of the material removed. The maintenance was not a heavy item, but it was considered advisable to have a man constantly in attendance for about 3 hours before and 3 hours after high-water at every tide during the 2 years the dam was in use. During all that time there was not the slightest leakage under the dam. At first there was slight leakage in places through the clay, chiefly at the bolt-holes, but after the clay had fairly settled down there was practically no leakage, unless at high spring-tides, when there was a certain amount of leakage under the caps. That, however, was easily dealt with by two 9-inch, belt-driven, centrifugal pumps, one placed at the mouth of No. 2 graving-dock and the other at the south-east corner of the outer basin. Those two pumps easily dealt with the drainage-water of the whole area, except on the night of the 26th November, 1905, when there occurred the highest tide ever recorded in the Mersey. It rose about 3 feet above extraordinary high-water mark, and as there was a considerable swell, every wave for nearly an hour swept over the whole length of the dam, making a waterfall about 900 feet long and 30 feet high, the beauty of which he was unable to appreciate. No damage was done, however, the rock bank behind the dam proving an excellent receptacle for the falling water. He noticed in connection with the southern embankment that the Author referred to the outer slope as being pitched. That was rather misleading, as the outer portion was formed of rock excavation, simply deposited from

side-tip wagons to protect clay from the wash of the water. There was only one point in the Paper on which he would like to express a difference of opinion, namely, the Author's statement that great economy had been effected by leaving in the rock sides and bottom of the graving-docks. In fixing an all-round price for that rock excavation, it had been necessary to consider the extra cost entailed owing to the careful manner in which the work had to be carried out. If permission had been given to take the rock out roughly, making provision for a thin lining of concrete, he thought it would have been almost, if not quite, as cheap; and there was no doubt that concrete altars and floors would have been more satisfactory than sandstone rock. Mr. Brodie.

Mr. ARTHUR MUSKER did not intend to discuss or criticize the Paper, but wished to add a few particulars in reference to the machinery for closing and opening the dock-gates. Seeing that there had been, and still was, considerable controversy as to the merits of electrical machinery *versus* hydraulic, it occurred to him it might be interesting to the profession to know the result of the working of electrical machinery in the present case. The construction of the machinery had been entrusted to him partly by the Mersey Docks and Harbour Board and partly by the Tranmere Bay Development Company. The Docks Board in the first place specified, and had fitted, hand-worked machinery only, but this was afterwards converted into electrically worked machinery by the Tranmere Bay Company. The hand-worked machinery was of an ordinary type, having opening- and closing-chains wound on barrels driven by spur-gearing, which in their turn were worked by capstan-bars. The gates were 91 feet wide and the chains were 1½ inch in diameter. These were outer gates or storm-gates, and considerable shocks came upon them, as would be realized when he mentioned that the chains had been broken. The machinery was specified to be stronger than the chains, and therefore received practically no damage. Afterwards, when it was converted to electrically driven machinery, spur- and worm-gear was added with motors, a slipping clutch being placed between the motor and the gear so that the motor should not be overloaded in any way. The worm-gear was added solely on account of want of space. This was therefore an instance of ordinary machinery, driven electrically, working under circumstances in which shocks were very severe; and it worked very satisfactorily, although personally he thought hydraulic machines were more adapted to rough work of that kind. Certain improvements had suggested themselves in the case of electrical machines of that type, namely, mechanical buffers Mr. Musker.

Mr. Musker. of some kind between the gear and the shocks. He thought there would be no difficulty in working them, any more than there would be with hydraulic gear. He did not know of any other electrically worked machines for dock-gates of that size, and if there were any he would be glad to hear of them. The power of the motors for working the 90-foot gates was only 15 to 20 HP., and the time of opening was 3 to 4 minutes.

Mr. Hudleston. Mr. F. HUDLESTON had listened to the Paper with some interest because, as an old pupil of the late Mr. G. F. Lyster, he knew the class of work that was done in Liverpool, and it was interesting to see the differences between the more modern graving-docks and appliances and those which were built 15 years ago. First of all, the cross section of the graving-docks at Tranmere Bay seemed to him to be rather too steep in the sides to give proper light and air to the bottom of the graving-dock. When he was at Liverpool the favourite dock of the time had a much flatter slope; and although he believed that in some of the newer docks the sides had had to be made steeper, it should be remembered that some of the older docks in Liverpool were nearly as steep in the sides, and they had gone out of fashion in favour of the flatter slope, which, giving much more light and air, enabled a ship to dry better, and consequently enabled work in the bottom of the dock to be carried out more satisfactorily. The next point he wished to refer to was what was described as the movement in the south wal lat the west end. As far as he could make out, the rest of the wall between the river and the crane was founded on rock. The inner part was on bearing-piles and moved outwards about 11 inches as a maximum. It was said that the wall did not cant over at all but slid at the bottom. That was interesting because in older practice there used to be a regular habit of building a long straight wall with a definite inward camber to allow for any movement that might take place in the settlement of the toe and the general pushing out of the wall, and thus to mask the ugly appearance of such movement. In the present case the whole wall appeared to have slipped out, and that seemed to him to be chiefly consequent upon the use of the bearing-piles. The diagrams (Figs. 4, Plate 2) showed three together without much rake, two towards the toe and one at the back. It appeared to him that it would have been rather a better design to have the front two closer together, because the mass of concrete could easily bridge the distance between, and all the pressure was on the toe. At that depth of excavation it was not easy to drive piles with a rake. It was not altogether to be wondered at that when the filling

Mr. Hudleston.

came in at the back of the wall there was a pushing-out generally and the wall, as it were, swung over on the piles, and bulged out into the dock. Mr. Musker's remarks about the machinery for the gates were very interesting. In common with many men who had been brought up in docks, he himself would prefer distinctly to have hydraulic machinery for gates, but he thought in the present case, where the large gates were seldom used —probably they were only opened very seldom, certainly not on every tide—it was not necessary to adopt expensive hydraulic machinery, because electrical machinery was distinctly cheaper; and moreover it was probably so made that hand-power could be applied at any time in the event of failure of the electric plant. He noticed caissons in the graving-docks, from which he gathered that the docks were not intended to be used very frequently. In Liverpool, in his own time, graving-docks invariably had gates, and they were especially used for painting vessels. Probably the Tranmere Bay graving-docks were used more for heavy repairs, when it did not matter whether 10 minutes were lost in getting the caissons set after vessels had come in or gone out.

Sir William White.

Sir William White, K.C.B., Past-President, observed that for him the Paper possessed great interest, because he believed he was right in saying that the works had been undertaken largely to give to the old-established firm of Laird at Birkenhead the power of dealing with modern ships both in building and in repair—ships which could not be accommodated in the historic yard where so much valuable work had been done since the commencement of iron shipbuilding. He was not aware of the conditions that had been laid down for the guidance of those designing the works, as to the scale of operations. So far as he could make out, from measurements on the plan, the utmost length provided for in building-slipways, clear of buildings, was about 700 feet, and he noticed that the entrance to the finishing-basin or repairing-basin was 91 feet 6 inches wide, and the effective width of the largest dock at the level of the sill was about 90 feet. Those dimensions, of course, were sufficient for anything in existence at the present time; but, for permanent works of such a nature, it appeared to him that the margin was rather small, when it was remembered that the new Cunard steamships were 88 feet in beam and that their design had been practically determined upon at the time the works at Tranmere were begun. Of course, it might have been decided that such vessels must be regarded as exceptional, and that it was wiser to keep to a more modest programme on the site in question and to allow the building of very large vessels to go elsewhere. The works were undoubtedly

Sir William White. very well proportioned: they all went together, the finishing-basin, the building-slips, and the docks; and it would be possible no doubt to lengthen the building-slips without much inconvenience, if it were so decided. He noticed that the depth of water provided at the outer ends of the launching-ways was about 7 feet 10 inches—and he thought it would be difficult, so far as he could judge from the drawings, to carry the launching-ways beyond the river-front without expensive work. When he had to settle points of a similar nature in connection with the design of the warship building-yard at Elswick in 1883, he did not hesitate so to pitch the ground or launching-ways—which he could place as he liked, because the original foreshore was under water at high tide, and it was necessary to make the ground—as to get 14 feet of water on the way-ends. In the working of that yard since, that depth had been found to be a great advantage in launching heavy armoured ships. It might be that to have secured such a depth as that in the Tranmere yard would have involved very much greater expense, because he noticed that with the 7 feet 10 inches the water appeared to come up the launchway about 150 feet at high water of spring-tides. There could be no doubt that the creation of such an important shipbuilding- and repairing-yard in the port of Liverpool ought to be of very great advantage, and he was sure the members of The Institution would wish every success to those who had had the courage to create that great establishment. In respect of repairs there was a very good opening in the port of Liverpool, with its vast increase of trade, for an establishment of such a nature; and with the modern equipment that would be there he was sure the old firm of Laird, which was now associated with the great Sheffield firm of Cammell, would be ensured a future which would be quite worthy of their historic past.

Sir Whately Eliot. Sir WHATELY ELIOT thought the docks and basins had been designed to take the fullest advantage of the materials at hand. The rock came up at the north end in a convenient way to receive the two graving-docks; in fact, the graving-docks were carved out of the solid rock. It was a pity the rock did not come up to the bottoms of the walls round the outer basin, which would have avoided the necessity of the piling, because he noticed the piling had already given evidence of being a weak point. Although it was hoped there would be no further movement of the walls, it was well known that in time the weakness was forgotten, heavy weights were put on the back of the quay-walls, and there was always a tendency for them to slip forward again. He hoped, however, that that would not occur. It had been interesting to hear the remark

Sir Whately Eliot.

made by Mr. Brodie about the shaping of the rock. Judging from his own experience he would say it was very tedious work and unsatisfactory. It was almost impossible to work rock, especially red sandstone rock, to a smooth and even face, and the patching it had received could not add much to the beauty of the work. He thought it would be interesting to know what were the actual proportions specified to the contractor for the concrete: it was said that it worked out afterwards at 1 part of cement to 8 parts of finished concrete. He had never been accustomed to state the proportions of concrete from the finished work in that way. The proportions given from experiments on the shingle as 1 of cement to 2·64 of sand and 6·32 of rough shingle must have been decided after the shingle was delivered on the spot, and those two proportions of sand and shingle mixed together would be nearer 8½ than 8. In the usual way it would have been called 8½-to-1 concrete instead of 8-to-1. The concrete piles seemed to afford a very advantageous way of getting over the difficulty of piling in ground where timber would not last any time. He had himself had difficulty in made ground overlying hard ground or compressed mud, in which piles would rot as fast as they were driven. The course he had had to adopt was to sink a shaft down to the top of the mud, drive piles and completely bury them, and carry out the rest in concrete. The Simplex method of piling overcame that difficulty by enabling the pile to be driven from the surface.

Mr. Brereton.

Mr. Cuthbert A. Brereton remarked that he had a recollection of the sandstone rock in the Tranmere district when the "Great Eastern" was on the gridiron at Rock Ferry in 1862, and he had had experience in another place of a graving-dock cut in sandstone rock, where it was found, after some years of use, that the fact of the face of the rock becoming alternately wet and dry caused the surface to disintegrate by degrees and rendered it very difficult to secure the shores satisfactorily. He would be very glad to know whether anything of that sort had occurred at Tranmere. The cost of cutting the rock to the exact shape required must have been heavy, and he thought it would have been better to face the rock with either concrete or thin slabs of stone. Another point he wished to ask was whether much water had been met with in the graving-docks—water that had had to be dealt with by pumping. In some places sandstone contained a great deal of water, but at Tranmere there might not be much, as the water from the back of the wall seemed to have been cut off, and consequently there was probably not much pressure upon it. As to the bearing-piles for the buildings, the system of driving a hollow pile no

Mr. Brereton. doubt had its advantages where the ground was soft, and where dependence was placed on skin-friction more than upon direct bearing-power, because when the "form" was drawn up in anything like loose ground there would be a tendency for the ground to fall in and a less satisfactory bearing-point would be obtained than when the pile itself was driven down to the solid. He desired to know whether any case had occurred of inequality of settlement, although perhaps there had not been an opportunity of ascertaining that, as the heads of the piles were buried in a kind of platform of concrete. Still, there might have been isolated cases where such experience had been obtained. With regard to the movement of the wall, similar movements had occurred in other places, where a good foundation was left for a bad one. It certainly appeared as though the bearing-piles were hardly equal to their work, particularly in regard to the rake or batter given to them. When once a wall began to come forward the piles leant over with the motion and tended to send the wall over farther still. He would like to know why it had not been carried down a little deeper, so as to get a bigger footing to resist any forward motion. The general section of the wall was the same as that founded on the solid rock, whereas it was customary to have a greater thickness at the base when on a soft foundation or on bearing-piles.

Mr. Baterden. Mr. J. R. Baterden noticed that the coffer-dam, as was usually the case, had been made with a double row of piling, and he wished to know whether the engineers had had any experience with a single-pile dam. He had had to do with seven or eight graving-docks, and in only one case had a double row of piles been put in; and curiously enough that had been the only case in which there was the slightest difficulty with the coffer-dams. A weak point of double coffer-dams was the impossibility of knowing what was going on inside. The clay settled, and the water travelled along the bolts; whereas with a single-pile dam a leak was easily seen. It appeared to him to be wholly a question of strutting. He was particularly surprised at seeing that the rock altars had been left in their natural condition. He had had to do with similar work in hard shale, but the altars and also the flooring were faced with a thin layer of concrete. He was afraid that when the Tranmere graving-docks came to be used the soft sandstone would be found to wear away soon. Anyone who was acquainted with the work of graving-docks, and knew the damp condition of the floor, would feel quite certain that the sandstone in time would weather, and with men walking about on it and dragging plates and material over it, it would quickly deteriorate. The extra labour in dressing

altars to the exact face would have been fairly counterbalanced, and it would have cost very little, if any, more to put in a facing of concrete. Mr. Hudleston had referred to the width of the graving-docks at the top. Mr. Baterden's own experience had always been that ship-repairers did not like wide-topped docks. They were undoubtedly of advantage in drying the ships, but they caused a little more difficulty in shoring. In passing a graving-dock on the north-east coast recently he noticed a ship jammed against the top altar, only about 3 feet from the coping, so that it must have been rather dark down below. He wished to know whether there was any particular reason why caissons had been fitted instead of gates for the graving-docks. It might have been necessary to put a short gate and a long gate, but his own experience of caissons had been that they were a great trouble in a waterway, bobbing up and down and seriously inconveniencing shipping. The most serious objection to them was that the dock could not be used so quickly with caissons as with gates. The Paper said that the caissons would be floated to clear the grooves when there was 23 feet on the sill. It seemed to him there was only a few inches more than 23 feet on the sill of one of the docks at neap-tides, so that, unless he was mistaken, it was quite possible there might be an exceptionally low neap-tide, when great difficulty would be experienced. It was possible to open gates as soon as the water inside was level, but that was not the case with caissons. One advantage they had was that a line of railway could be taken over them, but that was the only advantage, and as a rule they were not cheaper than gates. The Simplex system of piling was interesting and novel. He was surprised to find it worked out so cheaply. In getting prices he had found that it could not hold its own against ordinary piling; in fact, he knew of one case where it could not hold its own even against greenheart piling. Mr. Baterden.

Mr. C. H. Colson noticed in the Paper a statement that the cement delivered on the works was not turned; apparently it was used just as it came from the manufacturer. That was not the usual practice on works of the kind, and if no ill effect on the concrete had resulted it tended to show that turning and aeration were to some extent unnecessary. He would be glad to know if any cracks or other signs of deterioration had been seen in the concrete. Mr. Colson.

Mr. Druitt Halpin asked whether it would not have been possible to reverse the arrangement shown in Fig. 1, Plate 2. The docks seemed to be chiefly for repairs, and where such large masses of material had to be moved about, it might have been worth consideration whether a great deal of carriage could not have been saved. All the material that came to the graving-dock probably had to be put into barges Mr. Druitt Halpin.

Mr. Druitt Halpin. and ferried across the dock and unloaded, whereas if the graving-docks had been put at the south side next to the shop, a great deal of useless transport might have been saved. With regard to the steepness of the walls in the graving-docks, he certainly thought there was a disadvantage on account of the absence of light and air, which was essential to iron ships of the present day. In painting and cleaning, light and air were of the greatest possible value. With regard to the disadvantage of having wide docks, he did not see any advantage in docking with long shores. He had docked over a thousand ships without using a shore at all, and he did not see why it should not be done in the docks under discussion.

Mr. Wood. Mr. JOHN T. WOOD observed that, while he would like the Author to reply to the criticisms, he might offer some remarks on one or two points. Mr. Brodie had disagreed with the Author on the question of concrete in the graving-docks, and his own opinion was and always had been Mr. Brodie's opinion. Owing to the laminated character of the rock in No. 1 graving-dock, since the Paper was written further excavation had been made and a concrete floor had been put in. In his own view there was not much objection to the altar-courses being in sandstone. He had had considerable experience for many years in the adjoining yard of Messrs. Laird Brothers, where the altar-courses in all the old docks, of probably 50 or 60 years' standing, were sandstone, and they were still good and sound. The works under discussion were, of course, a commercial undertaking, not carried out with public money but built as a paying concern, and naturally it had been necessary to study economy. The economy of constructing graving-docks in sandstone was very great, and the dressing of the altar-courses in sandstone was not expensive. With regard to appearance, where it was necessary to cut out decayed sandstone here and there, or to block up fissures, in a ship-yard, he did not think a few blue bricks would make any difference 5 or 6 years hence. The object had been to provide a good sound job for commercial purposes. The reason why the graving-docks had been placed as shown was partly on account of the suitability of the subsoil, the rock, but more because they were in close proximity to the existing works of Messrs. Laird and to the workshops and machinery. With reference to Sir William White's remarks as to the length of the launchways, those at present constructed could be used up to 700 feet, but 1,000 feet would be available on the new extension of 15 acres. As to the width of the entrances, when it was first proposed to have two wide entrances in the outer basin he advocated that one of them should be 80 feet and the other 100 feet, and when it came to one entrance he advised 100 feet but failed

to get it, and he was sorry to say the 91 feet 6 inches had been adopted. It was intended that the docks should be for constructive purposes or for the repair of large vessels. Messrs. Laird Brothers had six or seven other graving-docks in their present yard, where no doubt repairs would be done, leaving the new works to be used chiefly for the construction of new vessels. Hence the use of caissons instead of gates for the two graving-docks. With reference to the concrete, the specification was 8 to 1 for the bulk of the concrete, and 4 to 1 for the facing work. Up to the present time no settlement whatever had been noticed. Mr. Wood.

The AUTHOR, in reply, desired to acknowledge his indebtedness to Mr. Wood for supplementing the information contained in the Paper as to the general scheme of the work, and to Mr. Brodie for the valuable details which he had contributed regarding the coffer-dam. With reference to the question of facing the rock floor and walls of the graving-docks with concrete or masonry, it was purely from considerations of economy that this had not been done; and the Author agreed with Mr. Wood that for the floor of the docks and the treads of the altars some form of facing would have been desirable. The face of the walls, on the other hand, had been dressed very cheaply, appeared to be durable, and was not rendered particularly unsightly by the patchwork found necessary. With reference to Mr. Hudleston's remarks, the profile of the graving-dock walls was nearly the same as in the latest Liverpool graving-docks, though somewhat steeper than, for instance, that at Avonmouth. Considerable light and air was admitted by the small altars at the top falling away in a flatter slope than the rest. With reference to the movement of the south wall of the basin, only a length of 200 feet at the end nearer the river was founded on rock, the remaining portion between that and the crane being on hard clay, where, also, no movement had taken place. The Author imagined that, in the length founded on sand, not only the wall itself moved forward, but also the stratum immediately underneath it, pushed forward by the weight of the filling behind the wall. No settlement of the wall took place, and no perceptible canting, so that it did not seem as if an increase in the number of bearing-piles, or a concentration of them near the toe, would have prevented the movement. With regard to Mr. Brereton's criticism of the section of the wall—the whole of the wall was designed as for a soft foundation, only one-fifth of its length being on rock. It had not been considered advisable to carry down the foundations any lower, owing to the quickly increasing quantity of water which was met with as excavation proceeded into the bed of sand. No doubt if the wall The Author.

The Author. had been laid out in the first place with an inward camber, the unsightly effect of the forward movement would have been minimized. Judging also from the way in which the wall by the crane-foundation had remained firm, it would seem as if substantial counterforts, spaced, say, 50 feet apart, might prevent any general movement. With reference to the proportions of the concrete, this for the mass work was gauged by measure at 1 part of cement to 8 parts of gravel. The proportions of 2·64 parts of sand and 6·32 of shingle were those obtained (as an average) by a series of experiments, from 8 parts of the gravel used. As to the possible effects of shores on the unfaced sandstone altars of the graving-docks, the Author would point out that in the case of ships of the size which would chiefly be docked at Tranmere, the ends of the shores would rest on the small altars at the top, which were formed in concrete throughout. The inflow of water into the graving-docks was generally very slight indeed, consisting of almost imperceptible weeps through beds in the sandstone, except in the case of the north wall of No. 1 dock, to the back of which tidal water had access on certain days of the month. Weep-holes were provided here, to prevent pressure on the back of the wall, and for a short time at high-water of spring-tides they ran freely. The total amount of inflow was, however, never more than the drainage-pump could deal with easily. Caissons had been provided instead of gates, in order that, if necessary, water might be held up in the graving-docks so as to keep a vessel floating at low tide. These docks being purely for repairing purposes, a little time lost in getting the caisson in place was not of first importance. Turning and aerating the cement had not been considered necessary except in the case of two cargoes which failed at first to satisfy the usual tests for soundness, but which after aeration proved satisfactory. All the rest had been used after 3 weeks' storage on the site; and no ill effects had been discernible from the customary turning being omitted.

Correspondence.

Mr. JOHN S. BRODIE observed that the different methods employed in reclaiming the site of the docks and basins from tidal influence afforded an interesting study, the value of which would have been enhanced by a return showing the comparative cost of each method. Presumably, the three types of dam adopted were intended to meet the changes in the geological formation of the foreshore. The timber coffer-dam was a good example of heavy timber work, but probably the clay filling between the two lines of close whole-timber piling would have been much better if it had been previously puddled, and then deposited and rammed in layers. The use of old steel rails in fortifying that part of the concrete culvert which was subject to great pressure during the reclamation works appeared to be open to serious objection, as the same end could have been attained by much less expensive methods. It was not clear why the half-timber sheeting used on one section of the sewer was not driven low enough to obviate the necessity of driving internal runners by hand and strengthening the gantry. Dressing the sandstone to profile in the graving-docks must have effected a considerable saving, but the advisability of this course must depend upon the geological formation, and the quality of the sandstone. It was rather surprising that electricity had not been adopted as the motive power for the pumps, seeing that a generating-station was in close proximity to the works. It might have been better, when piling the foundations of the basin-walls, to cross-cap each line of piles, and to introduce either longitudinal rails or girders of a suitable section, dogged to the cross caps, and fished together. Probably also a row of whole-timber close piling, driven as toe-piles to the face of the wall, and projecting up the face 4 or 5 feet, with longitudinal walings bolted to the sheeting, would have been the means of preventing the forward creep of the wall. The sandstone displacers appeared to have been fixed closer together than was usually found in good work. The Simplex concrete piling for the workshop-foundations appeared to have been very successful, and its cost moderate. Mr. Brodie.

Mr. A. E. CAREY observed that it was somewhat difficult to discuss the merits of different methods of construction without a comparison of the actual costs involved, and the absence of such Mr. Carey.

Mr. Carey. information detracted materially from the value of a Paper. With regard to the alignment of the graving-docks at an angle of 64° with the line of the river-wall and pointing up-stream, perhaps the Author would state whether in actual use the design had been found entirely satisfactory, and whether it ensured vessels being manipulated with the minimum of risk. No. 1 dock appeared to have been hewn almost entirely out of the solid rock. There was a precedent for this operation at Panama, where Mr. Eiffel had built a canal-lock, about 650 feet in length, excavated in the same way out of the solid rock, and lined, where necessary, with masonry. With regard to the construction of the outer basin-walls, a considerable length of these appeared to be carried on a foundation of sand, the wall being supported on pitch-pine piling. The West Pier at Newhaven was similarly founded on Baltic timber piling, creosoted, the work having been carried out in 1881–2. The timber work was now so much destroyed by the teredo, that the underpinning of the concrete wall had been taken in hand, and was being carried out in reinforced concrete. He had recently carried out, at Southwold, a river quay-wall about 1,200 feet long and approximately similar in section to the walls of the outer basin at Tranmere. The Southwold wall was vertical down to low-water level and below this battered, being 14 feet wide at the base. The base of the wall was inclined, and along its bottom edge a toe had been carried down to arrest lateral movement. The wall stood on alluvial deposits of a silty character, and tie-rods were carried from the wall to concrete piers at the rear. In the test which the Author detailed for the raw material for concrete, he appeared to have considered as sand everything that would pass a $\frac{1}{8}$-inch mesh. A large proportion of material passing through this mesh must have been small beach gravel. The ingredients which passed a $\frac{1}{8}$-inch mesh were to the larger ingredients as 1 to 2·4. Two of coarse sand to one of cement made a mortar which resulted in water-tight concrete when mixed with a suitable proportion of gravel, but Mr. Carey doubted whether water-tightness would be obtained by the mixture indicated by the Author, and probably the admixture of a further quantity of sand would have been advantageous. The weight per cubic foot of the concrete used on this work was 7 to 8 per cent. higher than would have resulted from the use of an aggregate of ordinary sand and gravel. The Author's remarks as to workshop-foundations carried on Simplex concrete piling were of much interest, as there had been hitherto so little experience of this method of construction. The objection which naturally suggested itself was the possibility that

voids in the concrete might be left when the tube was withdrawn, and that thus there would not be absolute certainty of securing the full value of the normal bearing-area of the piles. That this objection was not entirely unfounded was borne out by the Author's experience, which went to show that, in plastic ground, the driving of adjacent piles tended to deform concrete recently deposited by this method in their vicinity. The cheapness of this type of piling was notable, and for passing grout into a foundation some modification of the system might prove a valuable expedient. Mr. Carey.

Mr. A. R. Ellison stated that since he succeeded to the position of Resident Engineer, previously held by the Author, several alterations had been made, the details of which might be of interest. In the later foundations the method of filling the form of the concrete piles had been considerably altered. By the new method the whole quantity of concrete necessary for making the pile was first deposited in the form, the latter being then withdrawn with one movement, instead of, as formerly, being partially withdrawn after each bucketful of concrete had been tipped into the form. A considerable head of concrete was always necessary inside the form, to compensate for the difference in the inside and outside diameters of the form, and also to cause any interstices in the ground to be filled. The piles formed by this method (particularly in soft ground) had proved to be superior to those formed when intermittent drawing was resorted to; and a considerable saving of time was also effected. Owing to the graving-docks having direct connection with the river, some serious difficulty was experienced at first in replacing a caisson in rough weather. In order to obviate this, on the south side of each entrance a timber stop had been fixed on the inside caisson quoins 6 feet below coping-level; each of these stops projected 6 inches off the face of the quoins and was 3 feet deep, with a slight bevel towards the caisson-chase. The method of replacing the caisson was the following:—One of the greenheart stems was guided into the north caisson-chase and securely held there by rope tackles, the other stem was then moored approximately opposite the south caisson-chase. The sinking of the caisson then proceeded until the caisson had sunk to such a level that the free stem could take a bearing against the timber stop, the filling-valves were closed until the free stem had been fastened securely against the stop, and the sinking of the caisson was resumed, the timber stop guiding the free stem into the caisson-chase. No difficulty had been experienced in replacing the caissons since this method was adopted. In the case of the 1000-foot slipways the slope had been altered from $\frac{1}{2}$ inch to $\frac{7}{16}$ inch per foot. Mr. Ellison.

The Author. The AUTHOR, in reply, observed with reference to Mr. John S. Brodie's remarks as to the construction of the concrete sewer-culvert, that his more recent experience in reinforced-concrete work had tended to confirm his opinion that the rough system of fortifying employed was, under the circumstances, safer than any more elaborate and scientific method, though admittedly not as economical. The first point for consideration had been that the culvert should be absolutely water-tight, the second that the time of construction should be as short as possible; and under these essential conditions —working at low-tide only, and under the difficulties mentioned in the Paper—any ordinary system of reinforced-concrete construction would have been at a great disadvantage. The half-timber sheet-piling had been driven from the surface, as low as it would go, a bed of sand stopping any further penetration. When the trench was opened out and timbered, the weight on the back would have prevented the piles from being driven any deeper, even if it had been considered advisable to attempt it. He regretted to be unable to answer Mr. Carey's question as to the angle of the graving-docks with the line of the river-wall, but the presence of a strong eddy towards high water at this side of the Mersey would render comparison with any other locality misleading. In the particular beach-gravel used in these works, the proportion of sharp sand was unusually high, averaging, at a rough estimate, 75 per cent. of what would pass through a $\frac{1}{8}$-inch mesh.

3 December, 1907.

Sir WILLIAM MATTHEWS, K.C.M.G., President, in the Chair.

The Council reported that they had recently admitted as

Students.

Douglas Francis Adey.
Walter Andrews.
Raymond Brocklehurst Angus.
Herbert Crossley Ashworth.
Reginald Charles Atkinson.
Frank Ernest Baird.
Charles Gordon Barber.
Joseph James Barnes.
Harry Lautour Bazalgette.
Albert Cottrell Beard.
Maurice Graeme Beck.
Peter Bergheim.
Robert Eardley Beswick.
Job James Bevan.
Thomas Henry Bewlay.
James Ragland Birch, B.A. (*Cantab.*)
James Stanley Bissett.
Edmund Charles Blake.
William Ewart Blizard.
Douglas John Blomfield.
John Poland Bowen.
Stanley Victor Boxer.
Robert Boyle, B.Sc. (*Glas.*)
Francis Richard Chace Brown.
Herbert Southerden Burn, B.A. (*Cantab.*)
Melvill George Burton.
James Leonard Busfield, B.Sc. (*London*).
Harold Lennard Buttle.
Charles Patrick Cæsar.
Arthur Havard Montriou Campion.
Bertram Harry Carreras.
Edward George Casson.
Ho-Nang Alfred Chai.
Geoffrey Robert Lucas Chance, B.A. (*Cantab.*)
Edwin Francis Chappell.
Ernest Henry Child, B.Sc. (*London*).
Albert Victor Cole.
Edgar Hugh Collcutt.
Cuthbert Collingwood.
Joseph Arnold Cooksey.
Robert Cotton, B.Sc. (*Manchester*).
Francis Reginald Court.
Peter McLellan Cran.
Ralph Arthur Cullum.
William Andrew Cunningham, B.A. (*Cantab.*)
Gerald Curry.
Kenneth Samuel Curtis.
Thomas Charles Blagden Davies, B.Sc. (*Manchester*).
Lawrence Stanley Deane.
Norman Dennes, B.Sc. (*London*).
Alexander Dewar, B.A. (*Cantab.*)
Alexander Fryear Dickinson.
Oscar Dixon, B.A. (*Cantab.*)
Stephen Easten.
Percy Tucker Easton.
Ernest Frederick Edwards.
Tom Elce, B.Sc. (*Manchester*).
David Arthur Evans.
Percy Newton Everett.
Philip Henry Fothergill, B.Eng. (*Liverpool*).

Students—continued.

Alexander Anson Gardiner.
Harold Marriot Gell.
William Gott Gibson.
Stephen Glennie.
Frederick Samuel De Vere Gould.
James Arthur Gravett.
George Wilfrid Acland Green, B.Sc. (*Birmingham*).
John Greenhalgh.
Wilfred Eric Randall Gurney.
Ernest Arthur Hamilton-Smythe.
Bertram Charles Hammond.
Robert Gerrard Hampson.
Hugh Baxter Harland.
William Healey.
Edward Sancroft Hector.
Stanley James Higgs.
Claud Harold Hill.
James Herbert Hill.
James Edgar Hobbs.
Grenville Henry Hodgson.
William Houghton.
Percy Powell Husbands.
Charles Kenneth Imison.
Kenneth Lister James.
Alexander Jardine.
Windsor Llewelyn Jenkins.
William Percy Johnson.
Hugh Alban Jones.
Reginald Trevor Jones.
Harold Aveling Joscelyne.
Arthur Gwynne Kay.
Hamilton Fyfe Kedslie.
Owen Hugh Keeling, B.A. (*Cantab.*)
Harold Leigh Kenyon, B.Sc. (*Manchester*).
Lawrence William Kershaw, B.Sc. (*Birmingham*).
Gilbert Kilner.
Frederick Guy Langdon.
William Chappell Crocker Langdon.
Edwin Percy Larkin.
Robert Ernest Lemon, B.A. (*Dubl.*)
Henry Norman Letherby.
Robert Charles Lundie, B.A. (*Cantab.*)
Marcus Macdonald.
Lynedoch Archibald Mackenzie.
William McLaren.
Leopold Joseph Ferdinand Manes.
Arthur Seymour Marr.
James Idris Martin.
John Weyman Mason.
John Caunter Matthew.
Robert Peel Mears, B.A. (*Cantab.*)
Arthur Pemberton Methuen, B.A. (*Cantab.*)
Henry Empson Middleton.
Gilbert Henry Millar, B.A. (*Cantab.*)
Thomas Pennall Miscampbell.
Kenneth William Sanderson Mitchell.
Ernest Morgan.
John McGregor Morris.
Pra Kash Nath.
Paul Naylor.
Arthur Winfield Nightingale.
Lionel Frank Wilfred Nolan.
Raymond Joseph Owens.
Herbert John Paul.
Percy Isaac Payne.
Stanley Pearson.
Bertie Howard Penn.
Alwyn Tayton Pepper.
Robert Arthur Pereira.
William Acton Phillips.
Gower Bouverie Raynor Pimm.
Jorge Guillermo Porter-Makin.
Adolf Camden Pratt.
Thomas Victor Pring.
John William Herbert Rea, B.A. (*Cantab.*)
Eustace Meredith Rice.
Leonard Henry Richards.
Robert William Richardson.
Isaac Althorp Ridgway, B.Sc. (*Manchester*).
Douglas George Robb, M.A. (*Aberdeen*).
Horace Digby Roberts.
George Strafford Robertson, B.A. (*Dubl.*)
Cecil Napier Ross, B.Sc. (*Tasmania*).
Gordon Kenitz Rouquette.
The Hon. John Gordon Sandilands.
Alec Searle.
Gilbert Colville Shadwell.
George Cecil Shipster.
Martin Hubert Shorto.

Students—continued.

EDWARD LAMBART SLADEN.
ALEXANDER SLATER.
ARCHIBALD COLIN HAMILTON PARKER SMITH, B.A. (*Cantab.*)
ERIC ROSKILLY SMITH.
FRANCIS STAFFORD SMITH.
GAVIN HILDICH SMITH, B.Sc. (*Birmingham*).
RICHARD ALBERT BELCHAM SMITH.
ERIC HAMILTON SMYTHE.
THOMAS STANLEY SPITTLE, B.A. (*Cantab.*)
ALFRED BENJAMIN STREET.
WILLIAM STUART TAIT.
JAMES ARDERN TAYLOR.
THOMAS REGINALD TERRELL.
WALLACE HOMER THOMPSON.
JAMES BLACK THOMSON.
LAUNCELOT POTTER TIMMINS.
PHILIP TRUMP.
FREDERICK GEORGE TURNER.
JEAN HENRI VOGEL.
CHOTALAL HARJIVAN VORA, B.A. (*Bombay*).
WALTER PROTHEROE WARLOW.
ROGER DONALD WATERS.
FREDERICK SEYMOUR WHALLEY.
TOM M'CALL WHITE.
GILBERT BROOKES WICKHAM.
LIONEL ST. GEORGE WILKINSON, B.Sc. (*Manchester*).
CHRISTOPHER MANNERS WILLIAMS.
HERBERT GERAINT WILLIAMS, B.Sc., B.Eng. (*Liverpool*).
FREDERICK WILLIAM WONHAM.
HERBERT LEE WRIGHT.
RODNEY GEORGE WRIGHT-NOOTH.

The Scrutineers reported that the following Candidates had been duly elected as

Associate Members.

ALFRED ERNEST ABBOTT.
CHARLES WILLIAM ALEXANDER.
ROBERT DOUGLAS ARCHIBALD, B.Sc. (*Glas.*)
WALTER BAILEY.
LUCIUS AVELING BAKER.
FREDERICK ARTHUR BARNES.
ERNEST GEORGE BECK.
ROBERT ARTHUR BELL.
WILFRID BELOE, Stud. Inst. C.E.
JOSEPH BENT.
WILLIAM EDGAR BERRY.
ERNEST SPENCER BOURNE.
GEORGE WALTER MORGAN BOYCOTT.
ALFRED HENRY BRISTOW, Stud. Inst. C.E.
GEORGE TOWNSHEND BROOKE.
PHILIP PIGGOTT BROWN.
FRED ERNEST BUTTON.
HAROLD EDWARD BYRNE, B.A.I. (*Dubl.*)
WILLIAM MACGILVRAY CARGILL.
ROBERT BOYLE CHILLINGWORTH.
ALEXANDER FRASER MACDONALD CLARK, M.A., B.Sc. (*Edin.*)
CHARLES STUART DUDLEY COLE.
ROBERT HUGH COLLINGHAM, Stud. Inst. C.E.
TUDOR GARFIELD CULE, B.Sc. (*Wales*), Stud. Inst. C.E.
JAMES MICHAEL CUNNINGHAM, M.A (*Cantab.*)
ARTHUR PAUL DASHWOOD, Stud. Inst. C.E.
ARTHUR DAVISON, Stud. Inst. C.E.
TREVOR DENNIS, B.A. (*Cantab.*)
JAMES STANLEY DIGGLE.
JOHN HUGH DODD.
BASIL MICHAEL DUKE.
HENRY AUGUSTUS DUPRÉ.
ALFRED TOMLIN EAST.
STUART STRICKLAND MOORE EDE.
CHARLES NOEL EDGE, B.A. (*Cantab.*)
RONALD WILLIAM EDWARDS, Stud. Inst. C.E.
ARTHUR SYDNEY WELSH ELDER.
ROBERT ELLIOTT, B.Sc. (*Glas.*)
LUIZ DE MORAES GOMES FERREIRA.
WILLIAM JOSHUA FITCH.
JOHN RHODES FOULDS.
RONALD JOSEPH FRANCIS.

Associate Members—continued.

Reginald Alexander Frank, Stud. Inst. C.E.
Albert Edward Gardener.
Harald Burton Gates, Stud. Inst. C.E.
Louis Henry Armistead Gaunt, B.Sc. (*Victoria*), Stud. Inst. C.E.
Geoffrey Geoghegan.
Hugh Gibson.
Frederick Michael Green, Stud. Inst. C.E.
Charles Samuel Hainworth.
Frances Edward Harrison, Stud. Inst. C.E.
Percival Tripp Harrison.
James Henry Harford.
Vincent Hart.
Charles FitzGerald Harvey, B.A.I. (*Dubl.*)
James Hassall.
Frank Siegmund Hildesheim.
Joseph Lawrence Hill, B.A. (*Cantab.*), Stud. Inst. C.E.
Cyril Hitchcock.
George Clay Hubback, B.Sc. (*Victoria*), Stud. Inst. C.E.
Ian David Erskine Hughes, Stud. Inst. C.E.
Robert Arthur Humphrys, B.Sc. (Engineering) (*Lond.*)
Harry Beckingham Jenkins.
Francis Edgar Kanthack.
Robinson Friedrich Kesting, Stud. Inst. C.E.
Edgar Archdale Lees.
Arthur Leighton, B.Sc. (*Victoria*), B.Eng. (*Liverpool*).
John William Liversedge.
John McClure.
Charles Selby Lennox MacFarlane, B.Sc. (*Glas.*)
Frederick Liddell Macpherson.
Frederick Percy Manson, Stud. Inst. C.E.
Edmond George Gladstone Matheson.
Frank May.
Ernest Minors, Stud. Inst. C.E.
Cecil Arbuthnot St. George Moore, B.A. (*Cantab.*)
John Ambrose Abercromby Morrison, M.A., B.Sc. (*Edin.*)
John Joseph Murphy.
Cornelius William Myddleton.
William Muir Nelson.
Reginald Nicholls.
Geoffrey Hamilton Norman, B.Sc. (Engineering) (*Lond.*)
Arthur Palmer, B.A.I. (*Dubl.*)
Arthur Watson Pimm.
Richard James Redding.
Carl Price Richards.
Alexander Richardson, B.Sc. (*Edin.*)
Allan Nelson McInnes Robertson, B.A., B.E. (*Royal*).
David Cameron Robertson.
Hugh Brown Glyn Robertson.
Frederick Eaton Robinson, Stud. Inst. C.E.
Ernest Gravenor Rodwell, F.C.H.
Harold Berkeley Rowlands, B.E. (*Sydney*).
Bernard Royce.
Henry Charles Sims, Jun., Stud. Inst. C.E.
Alexander Irving Sleigh, F.C.H.
Roland Harry Streatfeild.
Herbert Charles Swayne, Stud. Inst. C.E.
Shepherd Hoad Taylor.
Thomas Edward Thain, B.Sc. (Engineering) (*Lond.*)
John Milne Thorburn, B.Sc. (*Edin.*) Stud. Inst. C.E.
Arthur Trevor-Roberts, B.Sc. (*Victoria*), Stud. Inst. C.E.
Herbert Charles Wildish.
Harold Craigie Williams, B.A. (*Oxon.*)
Andrew Williamson.
Charles Clement Wilmot.
Ernest Charles Winter.
Owen Thomas Wood.

Associates.

John Henry Grindley, D.Sc. (*Victoria*).
Frederick Richard Smith.
Philip Osbert Gordon Usborne Capt. *R.E.*
Philip David Warren.

(Paper No. 3695.)

(Communicated from the National Physical Laboratory.)

"Experiments on Wind-Pressure."

By THOMAS ERNEST STANTON, D.Sc., M. Inst. C.E.

METHODS OF OBSERVATION.

THE experiments described in this Paper form the second part of the research on the distribution and intensity of the pressure of the wind on structures, which was proposed by the Committee of the National Physical Laboratory as the first investigation to be undertaken in the Engineering Department, and was commenced by the Author in 1902.

The first part of this research, of which the results were communicated to The Institution in December 1903, was the investigation of the resultant pressure and the distribution of pressure on flat plates normal or inclined to the direction of a uniform current of air.[1]

The chief reason for this preliminary work was to determine, if possible, the effect of the form and dimensions of the plate upon its resistance, since it appeared to be commonly recognized that geometrical form, or possibly actual dimensions, had considerable influence on the intensity of pressure. As it was seen that this determination would involve a large number of experiments made under precisely similar conditions, which would be very difficult to obtain in the open air, it was decided to make the experiments on small plates placed in a current of air artificially produced. For this purpose a vertical channel 24 inches in diameter was used, and

[1] "On the Resistance of Plane Surfaces in a Uniform Current of Air." Minutes of Proceedings Inst. C.E., vol. clvi, p. 78.

plates and models up to 4 square inches in area were exposed in this channel to a downward current of air produced by a fan.

The results obtained in that investigation indicated that, in a uniform current of air, and within the range in dimensions obtained, the resistance of geometrically similar plates was strictly proportional to the area of the plates, but that for dissimilar plates, such as long rectangles and circular plates, the resistances per unit area differed considerably. The value of the resistance so found, for round and square plates, was somewhat lower than that determined by Dines, Froude and Langley in their experiments, as will be seen from the following table, in which the values of the coefficient k in the formula

$$P = k V^2$$

are tabulated.[1]

Experimenter.	Method.	Value of k.
Dines	Whirling table	0·0029
Froude	Moving carriage	0·0036
Langley	Whirling table	0·0032
Stanton	Plate in uniform current	0·0027

In the discussion on the Author's previous Paper the feeling was expressed that experiments on a much larger scale in the open air should be made, because, although the resistance of similar plates in a uniform current might be proportional to the area, general experience tended to show that in actual winds, whose velocity was not uniform over either time or space, the mean pressure per square foot on a large surface was considerably less than on a small one. It was therefore decided to make observations on flat surfaces, of areas ranging up to 100 square feet, when exposed to the wind.

Previous Work on the Subject.—Records of the pressure exerted by the wind on flat plates 1 or 2 square feet in area have been made continuously for many years at various English meteorological stations. These records are made by instruments in which a pen, moving in accordance with, and proportionally to, the pressure on the plate, traces a curve on a rotating drum: in this way the time variation of the pressure of the wind on the plate is obtained. This record can then be compared with the curve of time variation of the velocity of the wind, taken from an instrument such as the Robinson cup anemometer, in order to deduce, if possible, the corresponding values of the pressure and velocity at any instant.

It is generally admitted by meteorologists that the records of pressure-plate anemometers leave much to be desired in point of

[1] P = pressure in pounds per square foot.
V = velocity of current in miles per hour.

accuracy, owing to the inertia of the plate and the other moving parts of the instrument, which renders them liable to register pressures greatly in excess of the true value. There appears, however, to be some probability that in many cases these instruments have been wrongly condemned, owing to imperfect knowledge of the motion of the air in winds.

The only experiments, so far as the Author is aware, which have been made on a plate of considerable dimensions, are those made at the Forth Bridge in 1882 and the following years. These were on a plate 300 square feet in area fixed in direction, and there were also two small plates, each 1½ square foot in area, one fixed in direction and the other free to revolve.

In his lecture[1] on the Forth Bridge to the British Association at Montreal in 1884, Sir Benjamin Baker summarized the readings of these gauges for 2 years by taking the mean of the maximum daily readings of the three gauges between 0 and 5 lbs., 5 and 10 lbs., etc., per square foot, and tabulating the results (Table I).

In all these cases it was found that each of the mean readings of the two small gauges was considerably higher than the corresponding mean reading for the large gauge. Taking the whole of the readings, the ratio of the revolving-gauge indications to those of the large-gauge indications is 1·5.

TABLE I.—OBSERVATIONS AT THE FORTH BRIDGE.

Range of Pressures.	Mean of Gauge Indications.		
	Revolving Gauge.	Small Fixed Gauge.	Large Fixed Gauge.
Lbs. per Sq. Ft. 0 to 5	Lbs. per Sq. Ft. 3·09	Lbs. per Sq. Ft. 2·92	Lbs. per Sq. Ft. 1·90
5 „ 10	7·58	7·70	4·75
10 „ 15	12·40	13·20	8·26
15 „ 20	17·06	17·90	12·66
20 „ 25	21·00	22·75	19·00
25 „ 30	27·00	28·50	18·25
30 „ 35	32·00	38·50	21·50
Above	65·00	41·00	35·25

Experiments on the relative resistances of moderately large plates in the wind have been made by Mr. W. H. Dines by the method of balancing the pressure on one plate by the pressure on a similar and

" [1] The Forth Bridge." London, 1884. Also *Engineering*, vol. xxxviii, p. 213.

smaller plate.[1] In these experiments a rotating shaft with it axis vertical carried the two plates, which were attached to th opposite arms of a light cross piece with an arrangement for varyin the distance of the plates from the vertical axis. In taking observa tions the plates, which were in one plane, were set normal to th wind, and the distance of one of them from the vertical axis varie until approximate balance of the two wind-pressures about th vertical axis was obtained. Using two plates, one 42 square fee and the other 9 square feet in area, Mr. Dines found that, i spite of considerable difficulty in adjusting the equilibrium, th pressure on the large plate was only 78 per cent. of that on th smaller one. Using plates 9 and 2½ square feet in area respec tively, he found that the pressure on the large plate was 89 pe cent. of that on the smaller one.

The results of both the Forth Bridge experiments and thos of Mr. Dines appear therefore to indicate that the wind-pressur on a large plate is of smaller mean intensity than that on a small plate. In discussing these experiments, however, it must b carefully noted that there is an essential difference between them. Sir Benjamin Baker's figures are the daily maximum indicated pressures of three gauges, which pressures may or may not have been indicated at the same instant. Mr. Dines's results give the relative mean intensities of pressure on two plates at any instant when there was an approximate balance.

Sir Benjamin Baker's explanation[2] of his results is that near the surface of the earth uniform velocity cannot obtain, and unsteady motion must be the rule, so that the threads of the currents moving at the highest velocity will strike an obstruction successively rather than simultaneously, and hence the mean pressure per square foot on a large area must be less than on a small area.

This variation in velocity in winds at the same instant of time has been verified experimentally by Mr. Dines,[3] who, by means of two velocity instruments of the same type, placed 11 feet apart, has found that the simultaneous velocities at these points bear a ratio to each other which may vary from 0·75 to 1·25. Mr. Dines also noticed, as an instance of widely varying conditions at these points 11 feet apart, that the liquid in one of his gauges would often be rising when that in the other was falling.

Further experiments fully confirming the observations of Mr.

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[1] Quarterly Journal of the Royal Meteorological Society, vol. xvi (1890), p. 205.
[2] Lecture at British Association Meeting, Montreal, 1884.
[3] Quarterly Journal of the Royal Meteorological Society, vol. xx (1894), p. 183.

Dines have been made by the Author, who has found that very appreciable differences of velocity may exist at the same instant at points whose horizontal distance apart is only 2 feet, but that in general the slope of the velocity-gradient is small enough to justify the assumption of practically uniform intensity of pressure over any single square foot.

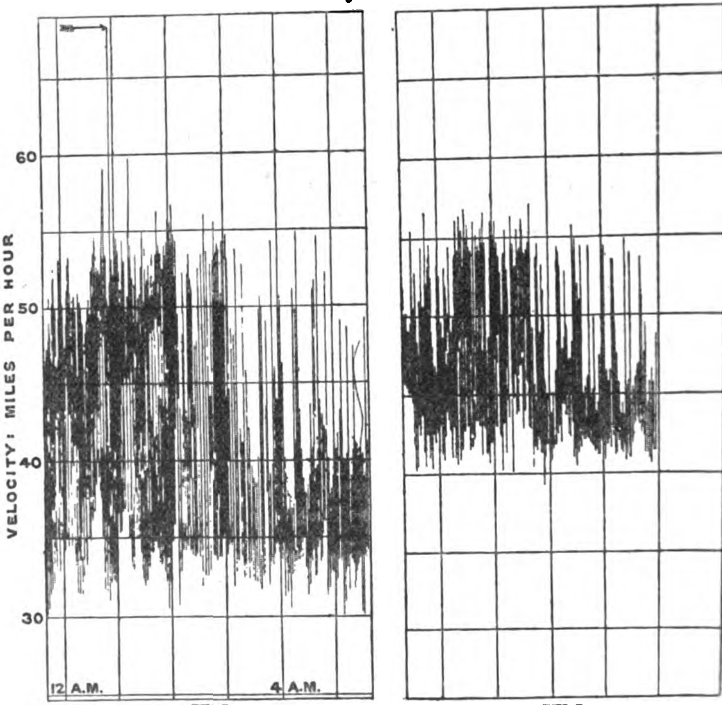
It will be seen that Sir Benjamin Baker's explanation of his results is strictly limited to the conditions under which his experiments were made, *i.e.*, that the pressures are the maximum pressures which have been registered in certain intervals of time; and further, it is assumed that the maximum wind-velocity which is attained in each of these intervals occurs at all points in the region of the pressure-plates, although not necessarily at the same instant. Under these conditions it is obvious that the small plate will register a pressure whose mean intensity is practically the maximum intensity attained in the interval, and also that the greatest mean intensity registered by the large plate will be less than this maximum value, owing to the varying intensity of pressure over its surface.

The importance of the foregoing assumption will be readily seen by considering the not impossible case of a gust of wind of short duration, whose intensity is much greater than the intensities of the gusts which precede or follow it in the given interval of time. Such a case is shown in *Fig. 1*, No. 1, which is a copy of the record made by the bridled anemometer at Holyhead on the 5th October, 1904, and is here reproduced by kind permission of Dr. W. N. Shaw, the Director of the Meteorological Office. According to this record the velocity of the maximum gust was approximately 68 miles per hour, or 13 per cent. higher than the next highest. Owing to the smallness of the time-scale it is not possible to determine the duration of this gust, but assuming it to be of a momentary character, as common experience tends to show, it is quite possible that in this gale the greatest pressure on a small gauge might be less per unit of area than the greatest pressure on a large gauge. The case of a sudden gust of this description is not a common one, but evidence of its occurrence is to be seen in one of the later records of the Forth Bridge experiments kindly furnished to the Author by Sir Benjamin Baker. On the 17th March, 1891, the maximum mean intensity of pressure on the large plate was $14\frac{1}{2}$ lbs. per square foot, whereas the maximum mean intensity on the small revolving plate was only $11\frac{1}{2}$ lbs. per square foot.

The common case, however, as proved by Sir Benjamin Baker's

experiments, is that in which the maximum gust is not greatly excess of others which occur in the given interval. Thus moderately steady gale will consist of a series of squalls, each squa being made up of a succession of gusts which gradually increase intensity up to a maximum and then decay, but no single squa

Figs 1.



No. 1. 5 OCTOBER 1904 No. 2. 8 OCTOBER 1904

RECORDS OF BRIDLED ANEMOMETER AT HOLYHEAD.

reaches an intensity greatly in excess of the others. Such a case is shown in *Figs. 1*, No. 2.[1]

It may be concluded from the foregoing evidence that the excess of the maximum recorded mean intensity of pressure on the small

[1] The bridled anemometer at Holyhead was designed by the late Sir George Stokes, and consists of five hemispherical cups attached to a vertical spindle by short arms, and so arranged that no cup acts as a screen to the others. The mechanism is designed so that the moment of the wind-pressure on the cups about the vertical axis is recorded on a drum. The records of this instrument have been chosen for illustration because it is more sensitive to momentary gusts than any of the other recording instruments in common use.

plate over that on the larger plate in Sir Benjamin Baker's experiments is not necessarily due to a purely dimensional effect of the plates, that is, an effect which would be equally apparent if the velocity of the wind were perfectly uniform.

Under the same conditions of variable velocity as discussed above, a large and a small plate, which were fitted with appliances for recording the minimum pressures in any interval of time, would give results in which the minimum mean intensity of pressure on the small plate would be considerably below the value of that on the large plate. This is evident, since the minimum mean intensity on the large plate will certainly be higher than the minimum pressure on any square foot of its surface. But if, instead of the recorded maximum and minimum pressures in any interval, the simultaneous pressures at any instant on two plates are considered, the result may be of a different character. In such a case the mean intensity of pressure on the small plate may be greater or less than that on the large plate, and apart from any purely dimensional effect, it may be assumed that, taking the mean of a sufficiently large number of observations, the average pressure for the two plates will be the same in value. Now, turning to the consideration of Mr. Dines's experiments, which consisted of simultaneous observations on two plates, it appears that the mean intensity of pressure on the small plate was always greater than that on the large plate. Under the above assumption it is evident that the explanation of those results cannot be found in the unequal distribution of the velocity of the wind at any instant. The cause of the apparent preponderance in intensity of pressure of the small plate over the large one must therefore be sought in other directions, and may be :—

(1) A real dimensional effect; or

(2) The possibility of eddies from the large plate affecting the pressure on the small plate, or the proximity of the ground affecting the large plate.

With regard to (1), it may be assumed that if a purely dimensional effect exists it will be found in the negative pressure set up by the eddies at the back of the plate, as it is difficult to suppose that the distribution of pressure in front of the board will be affected by the scale of the experiment. Its existence should in this case be easily detected.

With regard to (2), the Author's experience tends to prove that the pressure on a plate is considerably influenced by the pressure of any large area in its vicinity. This has been shown by balancing a small plate in a uniform current, in the manner of the Author's

previous experiments, and then moving another larger surface, which was already in the plane of the other plate, nearer to it. The result is always to increase the pressure on the small plate.

An examination of previous work would therefore appear to indicate that the existence of a purely dimensional effect in the resistance of plates cannot be regarded as certain, since Sir Benjamin Baker's results can be entirely explained apart from such an effect, and those of Mr. Dines may be due to other causes. The evidence of the Author's previous experiments in a uniform current is against the existence of any effect of this kind, which varies continuously with the dimensions of the plate.

As the further investigation of this matter seemed to be of the greatest importance, it was decided to make the determination of the existence or non-existence of the purely dimensional effect on the intensity of pressure the chief feature of the present research. As a consequence of this it became necessary to attempt either—

(1) The simultaneous determination of the pressures on two similar plates of different sizes; or

(2) The determination from a large number of experiments of the relation between intensity of pressure and velocity of the wind for each of a number of similar plates. The values of the constants so obtained would indicate the existence and magnitude of the dimensional effect sought.

As the first method appeared to offer a comparatively simple solution of the problem, some preliminary experiments were made on two similar plates, which were 50 and 5 square feet in area respectively, by attempting to balance the wind-pressure on one of them by that on the other, as in Mr. Dines's experiments. The plates were attached to a light frame on the top of a steel windmill tower 50 feet from the ground, and were arranged so that their centres of gravity were in a vertical line. The framework carrying the plates rested on a pair of knife-edges which could be adjusted in a vertical direction relative to the plates, which were at a fixed distance apart. It was hoped that, when the plates were set normal to the direction of the wind, it would be possible to adjust the knife-edge to a definite position in which the wind-pressure on one plate would balance that on the other.

Several sets of observations were made with this arrangement but without obtaining satisfactory results, it being impossible to determine any definite position of the knife-edges so that even approximate balance was ensured. The general effect was that several positions could be obtained in which sometimes the pressure on the small plate would predominate and sometimes that on the

large plate. In order to investigate the cause of this failure to secure a balance a number of pressure-holes were drilled in the two plates at corresponding points. Any two corresponding points, one in each plate, were then connected to the extremities of a delicate water-gauge and the difference of pressure was measured. It was found in this way that the wind-pressures at corresponding points were rarely the same in value, sometimes one predominating and sometimes the other. This result, which might have been predicted from the known variation in velocity of the wind, fully accounted for the difficulty in obtaining a balance between the resultant pressures on the two plates.

The method was therefore abandoned and the research was carried out on the lines of the second method, namely, determination of the pressure-velocity relation for each plate.

Description of the Experimental Appliances.—For the purposes of the experiments a steel windmill tower was erected in the grounds of the National Physical Laboratory in such a position that the prevalent winds, which are westerly and south-westerly, should encounter as few obstacles as possible immediately before reaching the pressure-boards. The situation was not particularly favourable for wind-pressure experiments, as it is more or less surrounded by the trees in Bushy Park; but in the above-mentioned directions there was practically open ground for a distance of 700 yards in front of the tower, with the exception of a wall $2\frac{1}{2}$ yards high and 15 yards in front of it. As the lowest edge of the largest pressure-board used was 38 feet above the top of this wall, it was not considered likely that the eddy from the wall would affect the pressure.

The tower was 50 feet in height from the ground to the cap, and consisted of four legs made of 2-inch by 2-inch by $\frac{5}{16}$-inch angles well braced together, and carrying a platform 5 feet 6 inches square 3 feet below the cap (Fig. 2, Plate 3). The framework of the tower was chosen as light as possible, in order to reduce the disturbances from eddies due to it, and it proved sufficiently stiff for the work, its only weakness being a liability to torsional oscillations about its vertical axis.

The tower was 10 feet square at the base, and the lower part, as shown in Fig. 2, was roofed in and used as an observing-station. A cross girder with a foot-step working in ball-bearings about a vertical axis was fitted to the cap of the tower, and to this was attached the frame carrying the pressure-board. This frame, also made as light as possible, was 15 inches deep and 10 feet square. The pressure-board rested on knife-edges fixed to the front of the frame so as not to be affected by the eddies from the tower. Two hand-

winches were placed one on either side of the tower, in order to raise and lower the frame when the pressure-boards were changed.

For the experiments on plane surfaces with normal impingement, three pressure-boards were used, one 10 feet square, one 10 feet by 5 feet, and one 5 feet square.

In order to reduce the weight as much as possible, the boards were made of mahogany $\frac{5}{16}$ inch thick, with a plane surface facing the wind and with stiffening ribs $1\frac{1}{2}$ inch deep at the back, so that the total thickness round the edges of the boards was approximately $1\frac{3}{4}$ inch. The 100-square-feet board was made in three parts, having a central portion 10 feet by 5 feet and an upper and lower leaf 10 feet by 2 feet 6 inches, which were hinged to the central part so that when observations were not being made these leaves could be folded over the central part, leaving an exposed area of 50 square feet. This was done because the tower was not considered strong enough to resist the severe pressure which might otherwise come on it in the event of a heavy gale arising.

Estimation of the Velocity of the Wind.—In the Author's previous experiments in a uniform current of air, the velocity of the air was calculated from the difference of pressure in two tubes placed in the current, one measuring the velocity head of the current—that is, it was an ordinary Pitot tube—and the other the static pressure of the current. The latter was a tube placed with its axis in the current, the end being stopped and small holes drilled in the side.[1]

The same arrangement was tried in the open-air experiments, but it was found that, owing to the rapid fluctuations in direction of the wind, the pressure in the static-pressure tube was by no means constant. For this reason this type was abandoned and the form adopted by Mr. Dines in his experiments was used. In this form the low-pressure tube is placed at right angles to the direction of the wind with its axis vertical, and there are holes drilled at short intervals all round it, so that any change in the direction of the wind will not affect the pressure in this tube. The arrangement of the two tubes used is shown in Fig. 3, Plate 3, and as the dimensions differ considerably from those of the one used by Mr. Dines, it was necessary to calibrate it in a current of known speed. This was done by putting it in the 24-inch experimental channel side by side with the form of Pitot tube and static pressure-tube used in the previous experiments, and whose constant was known. Simultaneous observations on both forms of gauge were taken at varying speeds of the current, and it was found that the ratio of the pressure-

[1] Minutes of Proceedings, Inst. C.E., vol. clvi, p. 82.

difference in the modified Dines gauge to the corresponding pressure-difference in the gauge previously used was fairly regular and equal to 1·54. By direct calibration from an air-meter[1] the velocity of the current in miles per hour by the gauge used in the previous experiments was found to be

$$V = 11 \cdot 90 \sqrt{R},$$

where R is the reading of the tilting gauge used for measuring the pressures. The corresponding relation for the modified Dines gauge here used is therefore

$$V = 9 \cdot 62 \sqrt{R}..$$

This agrees fairly well with the determination which Mr. Dines made for his own gauge by calibration on a whirling table.[2] In these experiments he found that at 40 miles per hour the difference of pressure in his tubes was

1·169 inch of water.

Put in terms of the tilting-gauge reading this relation would be

$$V = 9 \cdot 44 \sqrt{R},$$

which shows that the difference of pressure indicated by the original tubes of Mr. Dines and the tubes made for these experiments differ by less than 2 per cent. of the indications.

Method of Estimating the Pressure on the Board.—As the methods which have been previously used for obtaining the resultant wind-pressure on a plate have been much criticized on account of the possibility of their being affected by the inertia of the plate and its attached mechanism, it was considered very important to devise a method in which these inertia-effects would be so small as to be negligible. There was the further difficulty of transmitting the pressure-indications through a distance of 50 feet to the observing-table at the foot of the tower.

In order to overcome the difficulty of the inertia-effects the use of a thin steel diaphragm suggested itself, as in that way the actual movement of the pressure-board in a strong wind could be restricted to a few hundredths of an inch. After some preliminary experiments it was considered that the change in pressure of a given quantity of air contained behind the diaphragm in a cylinder and pipe leading to a delicate pressure-gauge at the foot of the tower could be estimated

[1] Minutes of Proceedings Inst. C.E., vol. clvi, p. 86.

[2] Quarterly Journal of the Royal Meteorological Society, vol. xviii (1892), p. 170.

and utilized as a measure of the pressure on the board by the following device. If two closed cylinders are connected, one to each leg of a U tube containing water, this water-gauge will not be sensitive to changes of temperature which are common to the whole system, but will respond to changes of pressure set up by the expansion or contraction of one of the cylinders.

The arrangement finally adopted is shown in Fig. 4, Plate 3, in which D is the casting forming the two cylinders bolted to the frame F, which carries the knife-edges on which the pressure-board B rests. The cylinders are divided by a partition-wall, each cylinder being bounded on the outside by diaphragms made of hard steel plate 0·03 inch thick.

The pressure-cylinder diaphragm carries a conical socket which faces a similar socket fixed to the pressure-board. The pressure is transmitted from the board to the diaphragm by the hardened steel pin P with small hemispherical ends. The object of this arrangement is to secure a perfectly direct central pressure on the diaphragm unaffected by small lateral and vertical displacements of the board, owing to lack of rigidity in the frame or oscillations of the tower in gales.

The whole casting is well lagged, and a pair of lead pipes T, from the pressure- and compensating-cylinders respectively, are carried down one leg of the tower, being carefully lagged with asbestos along their whole length. At the foot of the tower they are connected one to each of the auxiliary cylinders, C, which are also embedded in asbestos. From these auxiliary cylinders rubber pipes lead to the tilting water-gauge on which the pressures are measured.

The object of the auxiliary cylinders is two-fold: first, to allow the total volume of the air contained in the pipes and cylinders to be varied in accordance with the dimensions of the pressure-boards; and secondly, for the purpose of checking the calibration of the upper diaphragm from time to time after the first determination, and to detect the presence of leaks in the pipes. To enable this to be done, these cylinders also are provided with diaphragms, and to one of them pressure can be applied by a pin from the hand-wheel above, so that when once the gauge-reading for a given pressure on the upper diaphragm has been determined, the amount of the deflection of the lower diaphragm to produce the same gauge-reading can be read off from the hand-wheel and used for future calibrations. The gauge G is a simplified form of the one used in the Author's previous experiments on air-pressure.[1] Its principle is that of a U tube in

[1] Minutes of Proceedings Inst. C.E., vol. clvi, p. 82.

which the difference of pressure on the surfaces of the water in the limbs of the tube is measured by tilting the gauge through a small angle, so that there is no displacement of the water along the tube. In the original form of the gauge the observations were made on the surface of separation of the water and some oil contained in the horizontal limb of the U tube, but this arrangement, which rendered the gauge extremely sensitive, was not suitable for the rapid fluctuations in the pressure of the wind, which could not be followed by the wheel sufficiently quickly to prevent rupture of the surface of separation of the oil and water. For this reason a plain U tube was mounted on the tilting platform of the gauge, and during observations the hair-line of the microscope was kept as near as practicable on the surface of the water in one limb, and readings were taken only when the two were in coincidence.

It will be readily understood that the use of this arrangement was entirely dependent on the nearness in equality of the temperatures in the two sets of cylinders and pipes, since a quite small difference of temperature would be sufficient to mask the pressure-effects. By taking great care with the lagging it was found possible to make these temperature-effects small, and to obtain an estimate of their magnitude in the following manner. The effect of the slight differences in temperature showed itself in a gradual change in the zero of the water-gauge. The method of observation adopted was to take frequent zero-readings between short sets of pressure-readings noting the times of all the observations. At the end of the experiments a time-curve of zero-readings was plotted, from which the value of the zero for each separate pressure-observation could be scaled off. It was found that the amount of the correction varied considerably with the atmospheric conditions. In uniform cloudy weather the correction was quite small and it was only during intervals of sunshine that the zero-displacements were important.

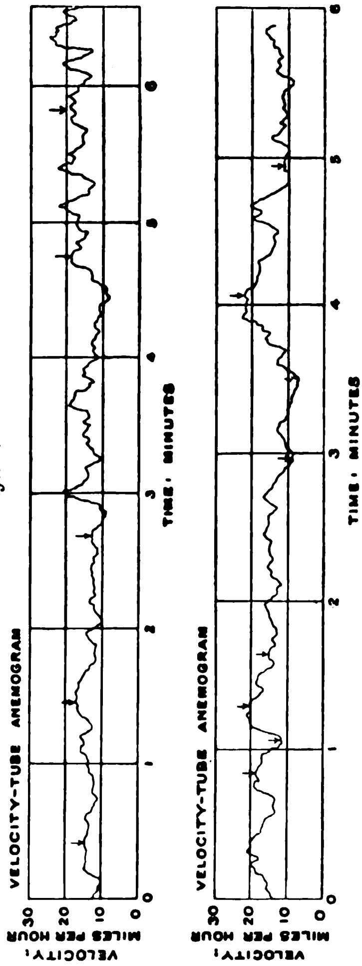
Calibration of the Diaphragm.—For the purpose of calibration known pressures were applied to the upper diaphragm, either by disconnecting it from the frame and loading it with weights when in a horizontal position, or by means of a spiral spring when fixed to the frame. The excess of pressure due to the deflection of the diaphragm was measured on the tilting gauge for each additional load, and was repeated several times up and down the scale until consistent readings had been obtained. The calibration-curve was then plotted (Fig. 5, Plate 3) and was used for the pressure-estimations in the actual wind-experiments.

Method of making Simultaneous Observations of the Velocity of the Wind and the Resultant Pressure on the Board.—With the arrange-

ments for the measurement of these two quantities on the two tilting water-gauges described above, the difficulties of making reliable simultaneous observations were as follows:—

(1) The possibility of a "damping" of the pressure- and velocity-indications owing to the length and small diameter of the connecting pipes.

Figs. 6.



(2) The existence of a time-lag in the indications of the gauges and the possibility that the amount of the time-lag might be different in the two sets of tubes.

(3) The possibility of a sufficiently rapid adjustment of the gauges not being made under the rapid fluctuations of wind-velocity which are known to obtain.

With reference to these difficulties it will be seen that they are all dependent on the time rate of variation of velocity of the wind: that is, if the velocity-conditions could be relied upon to remain steady for say 3 or 4 seconds, these difficulties could be successfully met. Now although the fluctuations in velocity of the wind in what is called a steady breeze are considerable in magnitude and take place with great rapidity, from a study of the available records of time variation in wind-velocity[1] and from observations made for the present research, it was considered that such intervals of practically steady conditions occurred sufficiently often to enable simultaneous observations of the kind required to be made with the apparatus described above. For the purpose of

[1] Quarterly Journal of the Royal Meteorological Society, vol. xx, p. 180, and vol. xviii, p. 175.

illustrating the existence of these steady intervals a record of the Dines anemometer at Kew Observatory was taken on a specially devised open time-scale, by permission of the Director of the Meteorological office, and is shown in *Figs. 6*. It will be seen that in those parts of the curve indicated by the arrows practically steady conditions obtained for intervals exceeding 3 seconds, and that these steady intervals occurred on an average about once a minute.

The difficulties mentioned in (2) and (3) were therefore considered to be successfully met by taking the simultaneous observations required in those approximately steady intervals; for although a small time-lag in the indications of the gauges was detected, its duration did not apparently amount to 1 second.

It was also found that the damping effect due to the long length of small tube was not appreciable when steady conditions had been maintained for 2 or 3 seconds.

A further difficulty in making the observations lay in the fact stated by Mr. Dines, that in two instruments situated 10 feet apart the velocity would be occasionally rising at one point and falling at the other at the same instant. This was met by the observers at the two water-gauges signalling to each other when the steady interval had been reached. It was only when the steady intervals coincided for the two gauges that readings were taken. In this method three observers were required, two at the table at the foot of the tower, who were continually watching the gauges and following the fluctuations of velocity and pressure as quickly as possible, so that the hair-line of the microscope was in approximate coincidence with the surface of the water, and one on the platform of the tower to carry out the periodical adjustment of the pressure-board to the mean direction of the wind, and to relieve the diaphragm of the pressure of the board when the zero observations were taken.

Results of the Experiments.

Experiments on the Pressure-Boards placed Normal to the Direction of the Wind.—In order to obtain a reliable value of the constant in the pressure-velocity relation it was found necessary to make about two hundred simultaneous observations of pressure and velocity for each board. The observations for the three boards used are plotted in Figs. 7, 8 and 9, Plate 3, the co-ordinates being the resultant pressure on the board and the pressure-difference in the velocity-tubes stated in divisions of the tilting-gauge scale. As the latter quantity is proportional to the square of the velocity of the wind,

these points should be approximately symmetrically grouped about a straight line passing through the origin. For the purpose of illustrating the extent to which this is fulfilled, the means of every ten observations in ascending order of velocity are indicated by the large circles, and it will be seen that the approximation is remarkably close considering the extreme divergence of the individual observations from the mean line. It will be noticed that the divergence is least marked in the case of the 50-square-feet pressure-board. This is perhaps due to the favourable wind-conditions under which the experiment on this board was made, and to the fact that the whole of the observations on this board were made in a single day, which was not the case with the other boards. No observations were rejected on account of possible errors in reading the gauges. The mean line was obtained by dividing the whole of the observations on each board into two groups of high-velocity and low-velocity readings respectively, and drawing the line through the two centres of gravity of the points in each group. This was considered preferable to joining the centre of gravity of the whole of the points to the origin, as this assumed an exact balance of the board about the knife-edges at zero velocity, which it was not possible to check, as there was always a certain amount of wind on the calmest days. These lines were found to pass through the origin in the cases of the 25- and 50-square-feet boards, but the line for the 100-square-feet board indicates a negative pressure of 3 lbs. at zero velocity, showing that the centre of gravity of the board was apparently overhanging the knife-edges by $\frac{3}{4}$ inch.

Correction for Difference in Elevation of Velocity-Tubes and Pressure-Boards.—As the centre of the pressure-board was 50 feet from the ground, and that of the velocity-tubes 15 feet higher, it was anticipated that there would be a correction to be applied to the readings of the velocity-tubes in order to reduce them to the corresponding values at the centre of the board. Owing to the importance of a fairly accurate determination of this correction, the arrangement of the two sets of velocity-tubes shown in Fig. 10, Plate 3, was devised for obtaining its value. The upper set of tubes was that used for the actual pressure-observations, and the lower set were placed at the same elevation as the centre of the board and sufficiently in front of the frame to prevent any effect from eddies set up by it. About two hundred simultaneous observations were taken from these two sets of tubes and the results are plotted in Fig. 10. The mean of sets of ten, in ascending order of magnitude, have also been plotted, to show more clearly the agreement of the results for all parts of the scale. The deviation of the mean line drawn through these plotted

points from the line drawn through the origin at an inclination of 45° to the base-line gives the effect due to difference in elevation, and indicates that the velocity of the wind at the velocity-tubes is 3 per cent. greater than that at the centre of the board. In the reductions of the experimental results which follow the values of the wind-velocity derived from the velocity-tube readings are corrected for the difference shown in Fig. 10.

From the positions of the mean lines in Figs. 7, 8 and 9 the values of the constant in the pressure-velocity relation,

$$P = KV^2$$

where P = pressure in pounds per square foot,
V = velocity of the wind in miles per hour

have been calculated for each of the three boards and are given in the following Table:—

Pressure-Board.	Value of K.
5 feet by 5 feet	0·00320
10 ,, ,, 5 ,, (long axis horizontal). . . .	0·00318
10 ,, ,, 10 ,, ...	0·00322

In addition to the determination of the resultant pressure, an independent estimation was made by measuring the intensities of the pressure at various points of the 10-by-5 board, on both windward and leeward sides; and the calculated pressures on the whole board from these data were found to agree with the results obtained from the diaphragm. Since the value of the constant in the pressure-velocity relation is practically the same for each of the three boards, it is evident that, for this range in size, any purely dimensional effect in the resistance, if it exists, is a very small one. This conclusion is further strengthened by comparing the results given in the above Table with the mean of the open-air experiments of Dines, Froude, and Langley on plates of much smaller dimensions, which is almost identical with the value given in the Table.

There appear, therefore, to be good reasons for supposing that the mean intensity of pressure on similar surfaces of areas greater than 1 square foot, exposed to the wind, is independent of their actual dimensions.

The only observations here cited which conflict with this view are those of Mr. Dines, whose combined experiments indicated that the pressure on a plate 42 square feet in area was only 70 per cent. of that on a plate of 2½ square feet. Now the value of the constant in the pressure-velocity relation, for plates whose dimensions are of the order of the small plate here referred to, has been found by

Mr. Dines to be 0·0029. The value of the constant for the large plate would therefore be 0·0020. Comparing this with the value 0·0032, determined in the present experiments for a plate of slightly larger dimensions, the Author ventures to think that, in the experiments of Mr. Dines, either the eddies from the large plate affected the small one, or the large plate was partially screened by adjacent objects or by the surface of the ground. As the lower edge of the large plate appears to have been only 8 or 9 feet from the ground, this latter influence may have been considerable.

There remains the comparison of the present results with those obtained in the Author's previous experiments on plates ranging from ½ inch to 2 inches in diameter in a uniform current of air. As the value of the constant in these experiments was 0·0027, the increase in resistance in the open air is somewhat marked, the ratio of the two constants being 1·18.

In discussing the two cases it will be convenient to make comparisons

(1) Between the pressures on the windward sides.
(2) Between the pressures on the leeward sides.

With reference to the windward pressures, it was found in the experiments in a uniform current on the distribution of pressure on the plates that the pressure in the centre of the windward side was approximately

$$\tfrac{1}{2}\,\rho\,V^2,$$

where ρ = density of current,
V = velocity of current,

and equal in value to the pressure in the Pitot tube. Corresponding observations on the 10-by-5 board showed that the pressure at its centre on the windward side was also approximately equal to the pressure in the Pitot tube of the velocity-gauge, and, further, the distribution of the pressure on the windward side was found not to differ very appreciably from that on a small model in the uniform current.

From this it appears that the difference in the resistances is not, in the main, due to the intensity or distribution of the pressure on the windward side, so that the explanation must be sought in the relative value of the pressures on the leeward sides. It was not found possible to make this determination directly, owing to the unknown value of the static pressure in the open air, which could not be estimated with sufficient accuracy for the purpose. For this reason it was decided to measure the difference of pressure between the centre of the windward side and the centre of the leeward side, both in the open

air and in the experimental channel. In order to obtain a comparison of these centre-pressure differences, a series of observations were made on the velocity-tubes and large board in the open air, and on the same tubes and a small plate in the experimental channel. The following are the mean results:—

	In Open Air.	In Channel.
Ratio of velocity-tubes indications to centre-pressure differences	0·94	1·08

These results indicate that for the same velocity of air and wind the centre-pressure difference in the open air exceeds that in the experimental channel in the ratio 1·15.

Since the pressures on the windward side are approximately the same, it is evident that the suction effect due to eddies is considerably greater in the open air than in the channel, which points to the existence either of a purely dimensional effect of the plates of the kind described above, or of some peculiarity in the flow of the air in the channel due to the mechanical arrangements for securing uniformity of flow. Against the latter supposition there is the evidence of the Author's calibration of the Dines tubes in the channel, which agrees with that made by Mr. Dines in his whirling machine. Assuming, therefore, that there is a purely dimensional effect, it appears, both from the Author's previous experiments and the present research, that this effect does not increase uniformly with the dimensions, as is seen in the steady value of the constant in the two cases. The following experiment throws some light on the problem. A plate 6 inches long and 0·3 inch wide was placed in the channel, and the pressures were measured on the leeward side at two points in its central line, one 0·15 inch from the end and one 2 inches from the end. It was found that the intensity of the negative pressure in the latter case was more than 50 per cent. greater than the negative pressure near the end of the plate, which was approximately the same in value as that on the leeward side of a square plate 0·3 inch by 0·3 inch.

This may be taken as an illustration of the marked increase in the suction-effect due to the long dimension of the plate. It appears most probable that the greater resistance of the large pressure-boards in the wind is due to this dimensional effect, although the present experiments and those of previous observers show that the increase in this effect with the area of the plate must be very small for plates exceeding 1 square foot in area.

Experiments on a Model Girder.—Owing to the marked difference in resistance between flat plates and lattice-work such as braced girders, it was considered desirable to make experiments on a model girder of this kind. The one made for the purpose was 29 feet

in length and 3 feet 7½ inches deep, and consisted of two equal flanges 9 inches by 1 inch, connected by a double system of lattice bars and verticals, as shown in Fig. 11, Plate 3. The total area exposed to the wind was 56·3 square feet. The model was mounted on the knife-edges used for the previous experiments, and about two hundred observations of pressure and wind-velocity were made in the same manner as for the rectangular boards. The results are plotted, together with the means of the observations, in sets of ten in Fig. 11. From the inclination of the mean line drawn in the figure, the value of the constant in the pressure-velocity relation is found to be $K = 0 \cdot 00405$.

For the purpose of comparing the resistance of this model in the wind with that of a very small similar model in the experimental channel, the latter was made in brass, the ratio of its linear dimensions to those of the original being $\frac{1}{42}$, so that its area was 0·032 square foot. This was placed in the channel, and from a set of experiments upon it, the value of the constant in the pressure velocity relation was found to be 0·00338. Now the value of this constant determined in the channel for round and square plates was[1] 0·0027, so that the ratio of the resistance of the small model to that of the small plate in the uniform current is 1·25, and the ratio of the resistance of the large model to the large boards in the wind is 1·26, which shows that the relative resistances of dissimilar surfaces are the same in the two cases.

Experiments on Roof Models.—In the experiments in a uniform current of air described in the Author's previous Paper, the intensity of the pressure at certain points in the central plane perpendicular to the ridges of small roof-models was measured, and the results were tabulated. These pressures were estimated on the outside of the roof only, and were stated relatively to the static pressure of the air in the current, higher pressures than this being stated as positive, and lower pressures as negative. The reason for this was that the models were completely closed on all sides, so that the pressure on the inside of the roof was independent of the velocity of the current, and could be taken as equal to the static pressure of the latter. It will thus be seen that the results given were applicable only to the case of a building so closed as to be unaffected in its interior by the velocity of the wind. The experiments showed that in such a case for all angles between 30° and 60° the suction on the leeward side of the roof was considerable, as had been previously pointed out by Mr. J. O. V. Irminger.[2]

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[1] Minutes of Proceedings Inst. C.E., vol. clvi, p. 94. [2] *Ibid*, vol. cxviii, p. 468.

These experiments on small models were not carried further, chiefly on account of the difficulty of measuring the resultant pressure on the roof of such small models without interference from the other parts; and it was thought that sufficient evidence had been obtained of the suction-effects on the leeward sides of roofs to justify experiments on a larger scale in the open air.

The model which was constructed for these experiments is shown in Fig. 12, Plate 3, and consists of two mahogany boards each 8 feet by 7 feet, mounted on two pairs of steel principals, so constructed that the two sides of the model could be set at any angle between 30° and 60° to the horizontal. One of these boards was bolted to the framework, and the other, on which the pressure-observations were to be made, was attached in such a way as to be free to rotate about an axis perpendicular to the ridge under the action of gravity and the force of the wind. As the actual movement about this axis would be very small, the component of the weight in the plane of the board was taken by the two flexible steel strips SS, and the position of the axis was fixed by attaching steel knife-edges to the board, working on V blocks bolted to the frame and held in contact by the spiral springs KK.

The axis was made perpendicular to the ridge instead of parallel to it, because of the unknown position of the centre of pressure of the wind relatively to the ridge. It was considered that its position could safely be regarded as being in the line perpendicular to the ridge bisecting the board, and therefore its distance from the axis of the knife-edges was known. The centre of the board was fitted with a hardened steel cone and pin bearing upon a diaphragm of the same kind as that used for the experiments on normal pressure, and the observations were made in precisely the same way.

In the first series of experiments made on this model the two vertical boards shown in Figs. 13, Plate 3, were attached to the framework, extending downwards from the eaves for a distance of 30 inches. The spaces between the two boards at the ridge and those at the eaves were made practically air-tight by means of canvas strips so connected as to leave the pressure-board free from constraint. The ends of the model were also closed by canvas sheets as indicated in Figs. 13, the part underneath being left perfectly open.

In making the experiments the pressure-board was used alternately as the windward side and as the leeward side, by rotating the model through 180°. This necessitated the use of two sets of velocity-tubes to avoid the delay due to re-erecting the pipes whenever a change in the side observed was made. These were situated at a height of 9 feet above the ridge and 14 feet in front of it (Figs. 13).

Sets of observations were made at three inclinations—30°, 45° and 60° to the horizontal, for both windward and leeward sides, and the means of them, in sets of ten, are plotted in Figs. 13.

In these experiments the total pressure on the diaphragm was the sum of that due to the weight of the board, which depended on the inclination of the roof, and that due to the wind. The pressures on the diaphragm due to the weight of the board were carefully determined in the workshop before erection on the tower, in order to obtain a check on the plotted results. The points plotted in Figs. 13 have as ordinates the total pressures on the diaphragm at the corresponding wind-velocities, so that for any particular set of observations, the mean line through these points should cut the line of zero velocity at a point whose ordinate is the pressure due to the weight of the board. It was found that in all cases the pressure so determined agreed very well with the pressures measured before erection. It will be noticed that the deviation of the means of the observations from the mean line is rather more marked than in the experiments on normally exposed plates, but this is to be expected from the known instability of the pressure on inclined surfaces.

The mean results may also be conveniently represented as pressure per square foot of the roof-surface at a wind-velocity of 20 miles per hour, as given in the following Table:—

Inclination of Roof to Horizontal.	Pressure in Pounds per Square Foot of Roof Surface at 20 Miles per Hour.	
	Windward Side.	Leeward Side.
60°	+1·35	+0·15
45°	+1·13	0·0
30°	+0·61	−0·16

With reference to the pressures on the windward sides, it may be noticed that at 60° inclination the pressure is somewhat in excess of the corresponding pressure for normal impingement of the wind, a fact which is in accordance with previous experiments on inclined plates. Even at an inclination of 45° the pressure is not greatly diminished.

The pressures on the leeward side are seen to be comparatively small and to consist of a small pressure effect at 60°, a small suction-effect at 30°, and practically zero effect at 45°. The explanation of these low values of the *resultant* pressure on the leeward board is to be found in the reduction of the pressure on the inner side of the leeward board

caused by the eddies from the windward board. The interior of the roof-model, as stated above, was protected from the action of those eddies at the ridge and at the sides, but was exposed to their action from the lower edge of the short, vertical board attached to the windward side of the frame. The reduction of pressure set up inside the roof-model by these eddies is greater than that which obtained in the outer surface of the leeward board when inclined at 60°, is equal to it at 45°, and less at 30°, as will be seen by inspection of the Table.

The foregoing results may be taken to apply to roofs supported on columns through which the air is free to pass, and in such cases there does not appear to be any necessity for taking into account the effect of the wind on the leeward sides.

In order to obtain results which might be applicable to roofs supported on walls, the roof-model was altered in the manner indicated in Figs. 14, Plate 3. The vertical board at the eaves on the windward side was taken away, and that on the leeward side was lengthened to a depth of 6 feet. In this way it was hoped that the reduction of pressure inside the model due to the eddies from the windward side would be prevented.

The results of the experiments on this arrangement by taking the means of every ten readings are plotted in Figs. 14. It will be seen from the results given in the following Table that this device had the effect of producing resultant negative pressures of high intensity on the leeward board :—

Inclination of Roof to Horizontal.	Resultant Pressures in Pounds per Square Foot of Leeward Surface at 20 Miles per Hour.
64°	−0·36
48°	−0·68
36°	−0·82

These suction-effects are of the same order as those observed in the Author's previous experiments on small models, and it may be pointed out that at the smallest inclination the suction-effect is greater than the pressure-effect on the windward side. Doubtless, still greater suction-effects could have been obtained by extending the vertical plate attached to the eaves on the leeward side still lower, but it was considered that sufficient evidence had been obtained to lay down the approximate wind-effects which a roof may be called upon to resist.

The conditions of wind-pressure in the foregoing case may be taken as roughly approximating to those of the roof of a building in which the windows and doors are open on the windward side and closed on the leeward side, so that there will be a pressure set up inside the

building and acting on the inside of the roof, the magnitude of which is unknown but which may be assumed to be a large fraction of the pressure experienced by the windward side of a plate upon which the wind impinges normally. There will also be a negative pressure on the leeward side of the roof, due to the eddies from the ridge and ends of the building.

The determination of the pressure inside a building due to open windows and doors would have been a matter of considerable difficulty with the roof-model used for these experiments, but as the value of the fraction stated above could be equally well obtained from a small model in the experimental channel, a set of observations of such a kind was made. The effect of openings in the leeward wall of the model was also observed and is here tabulated. In the latter case, as would be expected, the pressure inside the building depends on the ratio of the areas of the openings on the leeward and windward walls. The area of the openings in each wall was approximately 4 per cent. of its surface.

Angle of Roof.	State of Openings.		Ratio of Intensity of Pressure Inside Model Building to Maximum Intensity on the Windward Side of a Plate on which the Wind Impinges Normally.
	Windward Side.	Leeward Side.	
60°	Open	Closed	1·00
60°	,,	Half-open	0·67
60°	,,	Open	0·20
30°	,,	Closed	0·82
30°	,,	Half-open	0·49
30°	,,	Open	0·20

Further, since the negative pressure due to eddies on the outside is approximately the same as that on the leeward side of a plate upon which the wind impinges normally, it appears that the maximum suction-effect on the leeward side of a roof inclined at 60° may be equal to the resultant pressure on a board of the same area placed normal to the wind. In the case of a roof inclined at 30°, the maximum effect may be roughly 70 per cent. of this amount. These estimations were further checked by observations of the difference of pressure between the two sides of the leeward plate in the small model used.

The results of the experiments on roof models here described are, in the Author's opinion, too complicated to be expressed in terms of the

inclination of the roof, but the maximum values of the constant in the pressure-velocity relation may be conveniently stated for the actual cases treated:—

(a) Roof mounted on columns through which the wind can pass.

	Values of k. 60°.	45°.	30°.
Windward side	+0·0034	+0·0028	+0·0015
Leeward side		Negligible.	

(b) Roofs of buildings in which the pressure on the interior may be affected by the wind.

	Values of k. 60°.	45°.	30°.
Windward side	+0·0034	+0·0028	+0·0015
Leeward side	−0·0032	..	−0·0022

General Conclusions.

With reference to the experiments as a whole, the individual observations here recorded may be taken as illustrating the extremely complicated nature of the motion of the air in winds, which had been previously detected by other observers. There can be no doubt that a correct appreciation of the actual distribution of velocity in winds will go far to remove the suspicions which have been entertained about many of the published records of wind-velocity and pressure. As an example, it has been the custom to allude to as manifestly inaccurate[1] the Bidston Observatory records of the gales of 1871 and 1877, in which the maximum velocities of the wind as recorded by an anemometer, and the maximum pressures of the wind as recorded by a pressure-plate were, 79 miles per hour and 90 lbs. per square foot on the 9th March, 1871, and 80 miles per hour and 64 lbs. per square foot on the 23rd November, 1877.

As the Author has previously pointed out,[2] it is not sufficiently recognized that these are the records of two instruments placed 10 feet apart, and that it is quite conceivable that, during the maximum gust on the 9th March, the velocity of the wind at the anemometer may have been 9 per cent. less than that at the pressure-plate, and on 23rd November, 9 per cent. greater, which would account for the apparent discrepancies.

It will be noticed that these experiments have been made at quite moderate wind-velocities ranging from 5 to 30 miles per hour. This was due partly to the locality in which the observations were made, and partly to the large number of observations required, which rendered necessary the use of the prevailing type of wind which had an average speed of 20 miles per hour. Assuming that the

[1] *E.g.*, *see* Minutes of Proceedings Inst. C.E., vol. clxv, p. 95.

[2] *Ibid.*, p. 124.

relation between pressure and velocity here determined holds for the strongest gales—which there seems no reason to doubt—the Author is of opinion that these experiments indicate a fairly simple and accurate method of estimating the force exerted on any structure by wind-pressure. This method is based on the fact brought out in the experiments, that the ratio of the wind-pressure on a complicated structure such as a braced girder to that on a square board of the same area is the same as the ratio of the resistance of a small scale model of the structure to a square plate of the same area when placed in an experimental channel in a uniform current of air. The fact that the resistance per unit area in the wind is not the same as in the uniform current is comparatively unimportant, for when once the resistance of any surface of simple form in the wind is known, the correction to be applied to the results obtained in the experimental apparatus can be determined for all subsequent observations.

Thus, adopting this method in the design of structures, when once the resistance of a small model of the structure had been determined, and that of the structure itself had been deduced from it, there would only remain the estimation of the maximum wind-velocity which might likely be attained on the site of the structure. This estimation would be made from the records of a reliable anemometer on or near the site. In the case of a very large and important structure in an exposed situation, where the maximum record of a single anemometer might be considered to give an excessive estimate of the total wind-force on the structure, the simplest and best method would seem to be to erect several sets of pressure-tubes at certain points in the area considered, and to connect all these tubes in parallel to the same recording instrument. In this way a reliable estimate of the mean velocity over the area might be made.

In conclusion, the Author desires to express his great indebtedness to his colleague, Mr. Leonard Bairstow, for his invaluable help in carrying out and reducing the experiments, and in overcoming the not inconsiderable practical difficulties which have arisen in the course of the work. He also begs to thank Dr. R. T. Glazebrook, F.R.S., for the facilities which he has given for the purpose of the research and for the interest which he has taken in it.

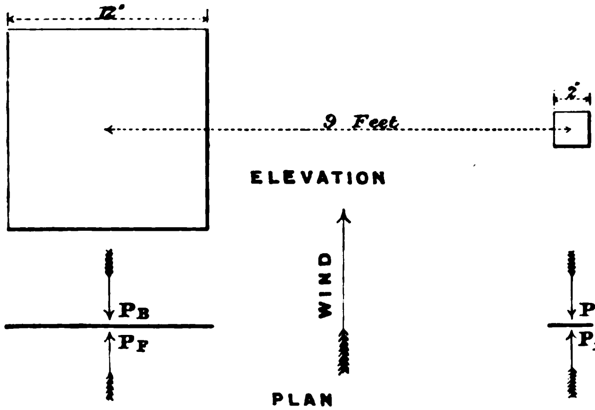
The Paper is accompanied by an Appendix containing Tables of the numerical values of the separate observations which may be referred to in the Library of The Institution, and by twelve sheets of drawings and two photographs, from which Plate 3 and the Figures in the text have been prepared.

Discussion.

The PRESIDENT, in moving a vote of thanks to the Author for his valuable Paper, said he was sure it would be the view of the members that thanks were also due to the Committee of the National Physical Laboratory, for permitting the results of the experiments to be brought before The Institution. The President.

The AUTHOR desired to state briefly the results of some observations made since the Paper was communicated, which afforded further evidence of the cause of the difference in value of the constants for large and small plates. He had put up on the experimental tower two plates, one 12 inches by 12 inches and one 2 inches by 2 inches, distant 9 feet apart (*Fig. 15*), which were connected to The Author.

Fig. 15.



the pressure-gauges at the foot of the tower, so that the intensities of pressure at the centres of the plates, both on the windward and the leeward side, could be simultaneously observed. Thus in *Fig. 15*—

P_F was the intensity of pressure at the centre of the windward side of the 12-inch plate.

P_B was the intensity of pressure at the centre of the leeward side of the 12-inch plate.

P_f was the intensity of pressure at the centre of the windward side of the 2-inch plate.

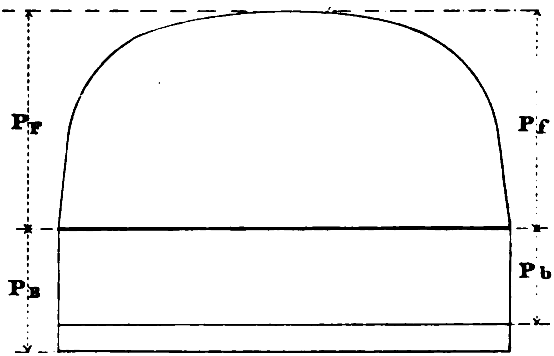
P_b was the intensity of pressure at the centre of the leeward side of the 2-inch plate.

The Author. By taking the mean of a sufficiently large number of observations it was found that whereas the windward pressures on the two plates were the same, as was to be expected, the reduction of pressure on the leeward side of the 12-inch plate was apparently greater than that on the small plate, and that the difference of these intensities of pressure was expressed by the relation

$$P_b - P_B = 0 \cdot 088\,(P_f - P_b).$$

The approximate distribution of pressure on the two plates was, therefore, that shown in *Fig. 16*, from which it followed that the resultant pressure per square foot on the large plate exceeded that on the small plate by the amount $(P_b - P^B)$. Now by careful observations on the distribution of pressure on small plates it had been found that the resultant pressure per square foot $= 0 \cdot 8$

Fig. 16.



$(P_f - P_b)$; so that denoting resultant pressures by the suffix r, since $P_R = P_r + (P_b - P_B)$, $P_R = 1 \cdot 11\ P_r$. It appeared therefore from this evidence that the resultant intensity of pressure on a 12-inch plate was 11 per cent. greater than that on a 2-inch plate. Taking the value of the constant $0 \cdot 0027$, determined by the Author for the small plate, that for the large plate would be $0 \cdot 0030$, which was in close agreement with the value of the constant for a plate of the same size determined by Mr. Dines. It would be seen that in the Paper the foregoing difference was traced to the same cause by measuring the centre pressure-differences of a 10 by 5-foot plate in the open, and a 2-inch-square plate in the experimental channel, but the evidence of the later experiments would doubtless be considered more conclusive, as in these the plates were under identical conditions.

Dr. CHARLES CHREE remarked that perhaps a few words as to how the Paper appealed to a meteorologist and physicist might be of some interest. What a meteorologist would like to know, when an apparent difference existed between the Author and Mr. Dines, was where the difference really lay. It might be one of two things. It might mean that when the two gentlemen experimented in the same wind they found different pressures, or it might mean that in the same wind they recorded the same pressure, but attached different values to the velocity. It might be a difference of 7 per cent. in the pressure they recorded, or it might be a difference of $3\frac{1}{2}$ per cent. in their estimate of the wind. Of course it was the latter point of view that interested a meteorologist, because if the difference existed and could be substantiated, it would mean that any measure of velocity based on Mr. Dines's scale would be altered if the Author's scale were adopted. Therefore the first thing to ascertain was whether the real difference was in the measurement of the pressure, or in the estimate of the velocity. There were one or two other points a physicist would like to have cleared up. What was the real meaning of the velocity V? It was very clear that neither Mr. Dines nor the Author really measured velocity; what they measured was pressure, and when they said that in a certain gale the velocity had a certain value they meant that the pressure was such that if the density of the air had a certain standard value the velocity would be so and so. Of course the density had not a constant value — generally during a gale it had a low value—so that it really meant that the estimate of the *velocity* based on a pressure-tube during a high gale was almost necessarily an under-estimate. It was an estimate of what the velocity would be if a certain standard density existed. The suggestion he had to make was that it would be very serviceable to physicists if an explicit statement were made as to what was the standard density. The density of air depended on the barometric pressure, on the temperature, and on the amount of vapour present. There were one or two references to meteorological matters in the Paper, one being an allusion to an apparent difference between results of different instruments at Bidston. These did not strike meteorologists with any great surprise because there were very extraordinary differences in the patterns; but what they would like to know was, which particular pattern of anemometer was used, and what was the nature of the exposures. There might be an instrument on the north of the building and another on the south of the building, and the exposure might vary so much that they would not be surprised at any difference which might be

Dr. Chree.

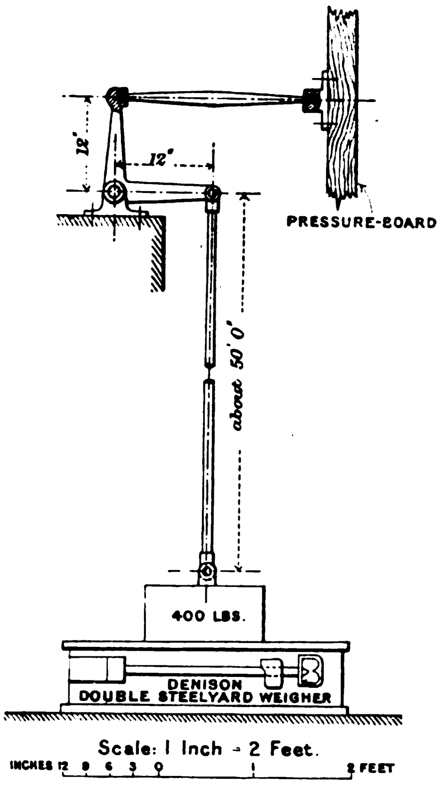
Dr. Chree. found. An illustration was given on p. 180 showing records of wind-velocity. The first one, with specially high ordinates was described as abnormal, and the diagram with a number of equal ordinates as normal. He had had a very large experience of diagrams of anemometers of various kinds passing through his hands, and he would not regard either of those diagrams as normal. He would regard something intermediate between them as much more common. One or two extremely severe gusts were very common indeed during certain kinds of gales, such as what was called a line squall, or the gales that accompanied thunderstorms. Even in a long gale—lasting perhaps about 12 hours—the usual feature was a large number of parallel crests, and there were only two or three crests which approached at all to the highest. Whether in the course of the 24 hours a large number of gusts approached the maximum depended very much on whether the heights of the successive crests were approximately equal. He would say that normally there were not many gusts whose velocity came within 10 per cent. of the maximum, which meant that the pressure would be only within 20 per cent. of the maximum. With regard to the influence of the dimensions—whether a large plate had a different effect from a small one—he thought a great deal would depend on the circumstances of the wind. A perfectly uniform wind was a very exceptional state of affairs, which was less interesting to engineers than the question of what would happen in a variable gale. In a variable gale not merely the intensity varied rapidly, but also the direction; so that a vane was continually shifting during a gale. Taking a large structure, such as the side of a building, it would probably be found that, quite apart from eddies, the wind at any instant had many different directions. That accounted for a difference between a large structure and a small one. With a large plate or structure, one of the dimensions might approach to what might be called a sort of measure of space in the wind itself. Successive gusts, or successive collections of gusts, might be regarded as answering more or less to waves succeeding one another at comparatively short intervals, and if the interval between the crests of successive waves became comparable with the longest dimension of the pressure-plate, that would certainly begin to influence the effect on the plate and to introduce a difference between a large and a small plate. He thought in practice it would be found that a good deal depended on the nature of the wind existing at the moment. Another point was this. Theoretically there ought to be a difference between a large plate and a small plate, when the wind was varying with the height

above the ground. Generally, near the ground the velocity increased with the height, but sometimes the converse occurred. But whether it increased or diminished with the height, the mean value of the pressure over a rectangular area would not be equal to the value at the centre of gravity; it would be perceptibly higher. He had made a little calculation for a rectangle and had found that the mean value exceeded the value of the pressure at the middle point of the height by one-twelfth of the pressure that would arise from a wind whose velocity was equal to the difference of the velocities at the top and bottom of the plate. Dr. Chree.

Mr. W. GILBERT observed that the essential details of the modified form of Dines gauge which had been used for measuring velocity were not shown in the Paper, but the general method was to measure the total water-gauge by one tube, and the static pressure by another tube, and the difference of the two readings gave the water-gauge due to velocity. It seemed from the results given in the Paper that apparently Mr. Dines's gauge gave too low a reading for the static pressure and therefore too high a reading for the gauge due to velocity. In the experiment cited a velocity of 40 miles per hour gave a difference of pressure of 1·169 inch, but if the static pressure had been measured correctly, the pressure-difference would have been about 0·78 inch. Of course, no error would have been introduced if the Dines gauge always under-estimated the static pressure in the same ratio, and the constant was known; but it would be preferable to use a form of gauge which measured the static pressure correctly. At p. 187 the Author described the simultaneous measurement of the pressure on large boards and of the velocity of the wind, the large boards being placed at the top of the steel tower. Perhaps it would have been better to measure the velocity of the wind opposite the centre of the large board, about 10 feet in front of it. Again, a board of smaller area, about 1½ square foot, might have been included, to give a greater range of sizes. With regard to the measurement of the pressures on the board—the pressures ranging from 20 lbs. to 170 lbs.—the Author's method was very ingenious, but the sources of error were still numerous. Possibly it would have been better to measure the pressure on the boards in a more direct manner. *Fig. 17* showed one method of doing that. A weight about twice as heavy as the quantity to be measured was placed on the platform of a small weighing-machine of the steelyard type, where the range of the platform was very small. The weight was connected to a bell-crank lever by a light tubular rod, as shown. A reading of the weighing-machine was taken before the Mr. Gilbert.

Mr. Gilbert. experiment began. The reduction of the scale-reading when the board was allowed to press against the bell-crank lever gave the pressure on the board. In that type of weighing-machine the movement of the platform was extremely small, so that no inertia-effect would come in. He himself had used that method of measurement where the conditions were similar. The Author's experiments had undoubtedly furnished very valuable results with regard to the distribution of pressure on small surfaces, and on roof-shapes. The next problem appeared to be the correct determination of the maximum wind-pressures in different localities in England and also abroad. Obviously a much higher value should be allowed for wind-pressure at exposed positions along the sea-coast than in inland towns. A general Table giving suitable values for various localities would be very useful, and in that connection an examination of buildings which, although apparently weak, had lasted for years, would no doubt be of great importance.

Fig. 17.



Dr. Shaw. Dr. W. N. Shaw had heard with much interest the Author's description of the extremely ingenious apparatus with which he had determined the pressure upon so large an area as 100 square feet with such a high degree of accuracy. The result which the Author put forward, namely, that the resistance offered to the wind by any structure in the open air was about 18 or 20 per cent. more than the resistance of the same structure in a uniform current of air—a result arrived at by putting together a large number of experiments

of various kinds—was one of extraordinary interest and obviously of great importance. It suggested a means whereby the effect of wind upon very complicated structures could be effectively determined, provided the second part of the problem could be adequately solved, namely, the determination of the maximum velocity of the air in a blast of wind. Having regard to the great interest that attached to this subject, the Author would pardon him if to a certain extent he took up the position of a cross-examiner upon certain points suggested by what was in the Paper. It was not that he wished in any way to suggest that the experiments had not been carried out with all necessary details, but that the question was of sufficient importance for some points to be made, if possible, a little clearer. One point—not perhaps a very important one—was that the Author called the apparatus with which he determined the velocity of the air a velocity-tube. An exactly similar apparatus was called by Mr. Dines, its inventor, a pressure-tube. It appeared to be a matter of nomenclature, but Dr. Chree had pointed out that in this particular case it was not altogether so. The direct effect upon which the velocity depended was the pressure of the air upon the mouth of the tube, and not, in the strict sense, velocity. Another point not very clearly made out was the direction of the wind with reference to the plate when observations were being made. He presumed that the plate had been fixed in direction and an occasion had been selected when the wind was blowing from an appropriate direction, and not that the large plate had been adjusted to the direction of the wind. Dr. Chree had also suggested a point upon which he would like to lay emphasis, namely, the possible effect of variation in the velocity of the air at different parts of the plate—variation of the velocity of the wind with height. Moreover, the pressure was measured by the thrust of the small spindle between the plate and the measuring-apparatus as shown in Fig. 4, Plate 3, and he presumed that the spindle was placed at the centre of gravity of the board. The question he wished to ask was, whether the resultant of the pressure upon the face of the board also acted at the centre of gravity of the board, or whether it did not act slightly higher. If it did, then he presumed that the effect of the apparatus in question would be to magnify to a certain extent the measurement of the pressure. Presumably such an effect would be of the same order as the variation in the magnitude of the pressure spoken of by Dr. Chree. Besides these points of detail, there were two passages in the Paper to which he wished to call attention; they dealt with the records that were obtained by means Dr. Shaw.

Dr. Shaw. of the oscillating pressure-plate, the Osler anemometer, which had given such very high records of wind-velocity at Bidston and elsewhere. On p. 177 the Author said that there appeared to be some probability that in many cases those instruments had been wrongly condemned owing to imperfect knowledge of the motion of the air in winds; and again in the general conclusions he referred to the subject in much the same sense. For his own part, Dr. Shaw did not wish to condemn the instrument, but simply to point out that there was a very remarkable divergence between the records obtained with the Osler oscillating pressure-plate and an instrument which did not oscillate in the wind. It was possible to arrive at the force which the wind exerted on a given area, either by the ordinary Osler oscillating pressure-plate or by a pressure-plate similar to the Osler, but which could not oscillate because, when the deflection was produced, a ratchet prevented the plate from coming back again. It was also possible to measure the same quantity with Mr. Dines's pressure-tube instrument, by means of a formula of conversion which the Author had verified. And now there was the Author's new ingenious apparatus, which was a new pressure-plate instrument not of the oscillating type—or if there were any oscillations they were extremely small. With that apparatus the Author had practically verified within comparatively small limits the factors that had been generally used for converting wind-velocity into wind-pressure. If there were differences they were of the order of 2 per cent., 5 per cent., or something of that sort. But when comparing the records of the ordinary oscillating pressure-plate with the records of a non-oscillating pressure-plate, or a Dines pressure-tube, converting velocity into pressure by one of the usual factors, then the differences obtained were of the order not of 10 per cent., but of 100 per cent.; that was to say, for the maximum pressure of a wind of 20 lbs. on one there might be obtained a pressure of as much as 50 lbs. on the other. The point he would like to submit was that there was now available a tolerably consistent system. Experiments had been made with anemometers at Holyhead, where the exposure was exceptionally good, and so far as could be seen, the results were consistent with the supposition that the pressure could be derived from the velocity by the use of a factor which did not differ much from 0·003. There remained the outstanding large pressures obtained with oscillating plates. They had not been explained by any structure of the wind or by any reason, except some peculiarity in the effects of wind upon an oscillating plate. Almost any single series of wind-experiments might be regarded with suspicion. Recently he had had the advantage of

having an anemometer exposed, by the courtesy of the Admiralty, upon the Rock of Gibraltar. Those who had seen the Rock, and were not particularly well acquainted with wind-observations, might consider that no exposure in the world for an anemometer was more ideal than a rock which stood so well up into the sky and was exposed to all the winds that blew. He had obtained records, covering some months, from the Rock, and it was found that while the wind might be blowing at 30 miles per hour from the west the anemometer would be recording a narrow oscillating trace of about 1 mile per hour; and when the wind was blowing from the east the anemometer would sometimes be recording a velocity of 20 to 30 miles per hour, with a series of lulls taking it down to zero very frequently. He hoped that the records might be made public very shortly, when it would be seen that what looked like a suitable position of an instrument for measuring wind did not necessarily afford a good exposure. Knowledge with regard to exposures at present was extremely uncertain. He did not mean to say that the exposures in any of the measurements that had been made were anything like so "bad" as the exposure at Gibraltar; but the precise effect of exposure still remained to be settled, and, until it was settled, any particular set of experiments might be viewed with a certain amount of suspicion. Consequently, every fresh set of experiments would be welcomed as a valuable addition to the subject, and he trusted the Author would be encouraged to proceed. The Author had put forward the view that the high pressures which Sir Benjamin Baker and almost everybody else had regarded as excessive were not necessarily so, because the structure of the wind was not fully understood. He sincerely trusted that the National Physical Laboratory and the Author would carry the experiments farther and determine whether the differences, as between 20 lbs. and 50 lbs. per square foot, were really to be accounted for by imperfect knowledge of the structure of the wind—about which much more was known now than formerly—or whether it was due to some peculiarity in the apparatus which had not yet been pointed out, but which he hoped the Author might be able to discover.

Dr. Shaw.

Sir WILLIAM WHITE, K.C.B., Past-President, observed that the discussion so far had proceeded chiefly from the point of view of the meteorologist, but of course the interest to The Institution was mainly connected with the application in engineering practice of the results to which the Author had drawn attention. While he did not wish to undervalue the remarks which had been made by previous speakers in regard to the possible influences, upon the observations made by the Author, of the details of the apparatus,

Sir William White.

Sir William White. the exposure, and other matters that were of great importance he ventured to call attention to the practical side of the question The first Paper read by the Author had been read during hi Presidency, and the subject being one to which, in the course of hi professional work, he had had occasion to give considerable attention he was very greatly interested in the details of the Author's method and his results; and since that time, by the courtesy of the Author he had had an opportunity of seeing at Bushy the continuance of th experiments on a larger scale. All the speakers had borne testimony— while suggesting possible improvements, or alternatives, or correction —to the great ingenuity and the wonderful attention to detail which had marked the construction of the apparatus, and he thought engineers would concur absolutely with meteorologists in that respect The Author had taken up the matter in the state in which it had been left by their lamented colleague, Sir Benjamin Baker, whose contribution to the previous discussion was one of the most valuable utterances embodied in that discussion; and he had given very good reason for thinking that mere increase in dimensions with certain proportions of a plane surface need not, under average conditions, involve any very sensible variation in the constant which would connect velocity and pressure. He thought the Author would agree that, whilst that was his present conclusion, it was open to revision; in fact, there were passages in the Paper which made that abundantly clear, and particularly was that true of a passage on p. 183, in which the Author said that the wind-pressures at corresponding points of two plates of similar form were rarely the same in value, sometimes one predominating and sometimes the other, and that that result might have been predicted from the known variation in the velocity of the wind. From other passages in the Paper it would be seen that the Author had been careful to place beyond doubt the basis of his conclusion as to the constant. From the engineer's point of view it seemed that there were two very important conclusions which might almost certainly now be reached by anyone who studied the two Papers of the Author. One was that a very close approach to accuracy could be obtained by the use of small models, if there was the power of connecting those small models with large-scale structures of similar form. That was particularly true in regard to the girder experiments. A photograph had been shown of the large lattice-girder that was tried at Bushy. He did not know whether the Author had with him the model girder which he tried in the experimental channel, but if he had, and would show it, so that it could be compared with the full-sized girder, which was

Sir William White.

29 feet long and 3 feet 7½ inches in depth, it would be seen what a weapon the method gave to engineering practice. [The AUTHOR exhibited the model.] The Author was suggesting to engineers, in connection with air-resistance and the effect of wind on structures, an experimental method which seemed to bid fair to take its place with what naval architects had long had the advantage of, in regard to the resistance of ships, in consequence of the success achieved by the wonderful mechanical skill of the late Mr. William Froude. Obviously it would be possible at the National Physical Laboratory, with standard patterns of girders, to get results tabulated in a way that would connect each type of girder with the resistance due to a rectangle which would contain it; and he hoped the Author would have the opportunity of going farther in that direction and of giving valuable data for future work. Sir Benjamin Baker, in speaking of his air-resistance experiments and about the allowance that he made for girders as compared with plates in his estimates of the effect of wind-pressure on bridges and girder-work generally, had told him that he had had recourse to simple pendulum experiments with model girders, and had obtained from them very valuable guidance and correction of his first impressions. The Author, in his previous Paper, gave a good deal of information on the variation of pressure over surfaces of different forms derived from his tiny models, and had now alluded to the subject again, showing how the pressure might vary on a rectangle which had a constant depth and a varying length. But the influence of form and proportion went much farther than that. It was not merely a question of rectangular surfaces but also of those which approached more or less to the triangular. The matter had a close connection with the effect of wind-pressure on sails in propelling ships, and it was in that connection that he himself had been drawn to look into the subject somewhat closely in past years. Although sails were now to a large extent obsolete, yet the subject was still one of very great interest, and of practical importance also, at all events for racing-yachts. With regard to one remark made by Dr. Shaw, he did not think it would be right to assume that the ratio of pressure in the open air and in a steady current of air had been made out to be as 1 to 1·18. He thought that what the Author said was that his old coefficient, obtained with his small models in a steady current of air, bore that ratio to the new coefficient he had deduced from the large-scale experiments in the open air. [The AUTHOR said that was so; it was merely an effect of dimensions.] Of course there might be other influences besides the current of air which went to

Sir William White. make up that difference. With regard to the analysis of pressures, the distinction between the practical constancy of pressure on the front of the plate and the variation in the negative pressure behind, and the effect on a sloping surface like a roof—all those matters were only the beginning of what might be possible with the experimental method that the Author had introduced, and he sincerely hoped with Dr. Shaw that it might be possible at the National Physical Laboratory to carry the series of experiments much farther.

The Author. The AUTHOR, in reply, remarked, with reference to Dr. Chree's criticisms, that in undertaking these experiments there had been no attempt to enter upon a meteorological research on the distribution of velocity in winds, except in so far as this distribution affected the resultant pressure on a surface exposed to the wind. His chief object had been to obtain the relation between the resultant pressure on a surface and some easily measured characteristic of the wind, so that engineers might be saved the trouble and expense of putting up large plates. The particular characteristic chosen was the pressure produced in the mouth of an open tube facing the wind. It was this relation which was shown in the plotted results, which were independent of the density, temperature, and humidity of the air, since both of the observed quantities would be equally affected by changes in these. It would therefore be seen that there had been no direct measurement of the velocity of the wind throughout the whole work; but as it was an invariably adopted convention to represent the intensity of the pressure of the wind on a surface in terms of the square of the velocity of the wind, the results had also been expressed in that way. For this purpose, when the experiments were completed the open tubes used were set up in a channel and their pressure-indications were observed when a current of air of known velocity was passed through the channel. These indications were then reduced to the values which they would possess when the current of air was at a pressure of 14·7 lbs. per square inch, and at a temperature of 60° F. The values of the constants given in the Paper would therefore refer to velocities under these conditions of pressure and temperature. He regretted that the description of the experiments given in the Paper should have led Dr. Shaw to suppose that they indicated any difference between the pressure on a plate in the wind and the pressure on the same plate when in a uniform current of air. The 18 per cent. difference to which Dr. Shaw referred was entirely due to the size of the plates used in the two cases. He had not been able to make observations of the resultant pressure on a 2-inch plate in the wind because the apparatus was not suitable for measuring such small pressures; but from observations

of the intensity of the pressure at certain points of the plate he was satisfied that the pressures in wind and uniform current on the same plate were identical. In reply to Dr. Shaw's question as to the deviation of the centre of pressure of the board from its centre of gravity, owing to the variation of velocity with height it was found (p. 191) that the velocity at a point 15 feet above the centre of the board was 3 per cent. greater than at the centre; so that, assuming the variation uniform, it could be shown that the centre of pressure would lie above the centre of gravity by approximately $\frac{1}{10}$ inch in the case of the boards 5 feet deep and $\frac{4}{10}$ inch in the case of the 10-foot board. The effect of this on the value of the constant would be inappreciable for the two smaller boards, and would reduce it for the 100-square-feet board by 1 per cent. With reference to Dr. Shaw's remark that "the Author had put forward the view that the high pressures which Sir Benjamin Baker and almost everybody else had regarded as excessive were not necessarily so, because the structure of the wind was not fully understood," he felt sure that no statement which could possibly have that construction put upon it could be found in the Paper. He had throughout regarded the accuracy of Sir Benjamin Baker's experiments as beyond question or discussion, and further, he had brought forward independent evidence obtained from his own observations, showing that these results must necessarily follow from the structure of the wind. The important question which had not been settled before these experiments were made was, whether the difference between large and small plates in the Forth Bridge experiments was due to actual dimensions or to the structure of the wind; and it was this question which at the outset he had decided to investigate. He had shown, he thought conclusively, that the dimensional effect for plates of more than 1 square foot in area was quite small, and that the difference in resistance was due to the structure of the wind, as indeed, Sir Benjamin Baker had predicted.[1] From remarks which had fallen from Sir William White and Dr. Shaw it appeared possible that his conclusions from the experiments as to the very small change with dimensions of the value of the constant in the pressure-velocity relation might not be regarded as consistent with the known fact that in any gale the maximum pressure registered on a large plate was considerably less than that on a small one. That they were perfectly consistent he had demonstrated in the Paper (p. 179), and the agreement would be further obvious from the consideration that the relation $p = 0 \cdot 0032 V^2$, applied to a board 50 square feet in area, referred strictly to the

The Author.

[1] Lecture at British Association Meeting, Montreal, 1884.

The Author. relation between the mean of a considerable number of observations of resultant pressure on the board and the mean of the corresponding squares of the velocities at the centre of the board. It must be clearly understood that this relation could not be used for the determination of the maximum or minimum resultant pressures, since it was certain that these would depend on the dimensions of the board.

Correspondence.

Mr. Airy. Mr. W. AIRY communicated the following history of the Forth Bridge experiments. After the fall of the Tay Bridge a Commission was appointed (of which he was secretary) to consider the question of wind-pressure on railway-structures. Sir John Fowler considered that the recommendations of the Commission contained in their Report were too stringent, and he was of opinion that for a given gust of wind the pressure per square foot on a large surface would be much less than the pressure per square foot on a small surface. And he considered this question to be so important in connection with the design of the Forth Bridge, which he then had in hand, that he determined to make these costly and troublesome experiments. He settled the size of the large board at 20 feet long by 15 feet high, as corresponding approximately to the area of the side of a railway-carriage. As it was practically impossible to arrange for so large a board to revolve so as always to face the wind, in the manner of the ordinary small pressure-anemometers, Mr. Airy erected it so as to face a little south of west, the direction from which the heaviest gusts might be expected in great storms: and a small revolving pressure-anemometer of the usual kind was to be erected for comparison with the large one. Mr. Airy suggested to Sir John Fowler the advisability of erecting also a small pressure-anemometer which should not revolve, but should face exactly in the same direction as the large one, and Sir John adopted this suggestion. Mr. Airy's reason was that he had often noticed, when watching the behaviour of small pressure-anemometers in high winds that they never did front the gusts of wind truly. They shied to one side when they felt the gusts, till they were brought back by their vanes, and then shied in the other direction, so that they were constantly on the

move. And he concluded that a fixed anemometer, even if it fronted the wind only approximately, would be likely to give higher results than a revolving one. As would be seen from the Table in the Paper, his conclusion was entirely verified by the results of the Forth Bridge experiments. Mr. Airy.

Major B. Baden-Powell observed that it would be interesting to know whether any observations had been made with regard to the horizontality of the wind. It was undoubtedly usual for the wind, especially near the ground or in the vicinity of trees, to have an upward trend. If, during these tests, such wind prevailed, it would cause the centre of pressure to be shifted from the centre of area, and would then bring an unequal strain on the pressure-board. That the wind was ever varying in exact horizontal direction was evident from watching a weather-cock; but such variations might be, so to speak, averaged. The upward trend, on the other hand, would be likely to be fairly steady, causing the centre of pressure to be always below the centre of area. Major Baden-Powell.

Mr. W. H. Dines was prepared to endorse fully the Author's statement that wind-pressure was strictly proportional to area when the shapes of the two areas were similar, and he agreed with the reasons the Author gave for thinking his own conclusion[1] to be wrong. However, in the discussion on that Paper Mr. Dines had said that it would be desirable to repeat the experiments, since the exposure in his case was very bad. Recently he and others had had occasion to send up a number of small spherical balloons. These balloons had in many cases been followed with two theodolites till they reached elevations of 10,000 feet or more. In many instances the rate of ascent had been calculated and compared with the diameter and lifting power, and the evidence so far obtained showed that the resistance to these balloons varied as the square of the diameter. He thought very great credit was due to the Author for his arrangement of the experiments, more particularly for his plan of measuring the pressure upon the plates. The only point on which he was doubtful was whether the Author's uniform correction of 3 per cent. for difference of elevation was allowable. Judging from the result of 5 years' constant kite-flying, it appeared that the velocity at 1,000 feet elevation (at inland stations such as Bushy) was quite double that at the surface, but there was certainly no regular rule in accordance with which the increase occurred. Sometimes the transition from a light to a strong wind was abrupt, sometimes it was gradual, and sometimes, especially on sunny Mr. Dines.

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[1] Quarterly Journal of the Royal Meteorological Society, vol. xvi, p. 206.

Mr. Dines. days in the spring and summer, there was no appreciable increase of velocity with elevation. If the 200 observations from which the 3 per cent. was deduced were spread over a large number of days the mean values would not be affected; but if they were concentrated upon a few days, he thought this might partly explain the discrepancies in the value of K. The percentage difference obtained over a house 30 feet high between anemometers at 71 and 50 feet was largely in excess of 3 per cent.[1] It was probably on this account that the Author had been unable to balance his two plates about a horizontal axis. On one point Mr. Dines could not at all agree with the Author, namely, in his belief in the accuracy of the Bidston and similar records. The discrepancy between the two anemometers was easily explained; it was on quite other grounds that Mr. Dines disbelieved in these high pressures. Had 90 lbs. per square foot really occurred it was not likely that the Observatory would have been left to tell the tale. Very good evidence would be required to establish such a pressure, and the instruments afford no evidence at all, being utterly unsuitable and worthless for recording maximum pressures. He had shown experimentally[2] that the records of an oscillating pressure-plate such as that in use at Bidston were largely dependent on the momentum of the moving parts. But there was now plenty of evidence to show that wind-pressures exceeding 30 lbs. per square foot were very rare in the British Isles. Pressure-plates in which the effect of momentum was eliminated had been in use for many years at both Holyhead and Southport, and it was very rarely that pressures exceeding 20 lbs. per square foot were recorded, although both stations were very exposed. On the other hand, at Greenwich, an inland station, with one of the oscillating plates 20 lbs. was not at all unusual. Pressure-tube anemometers were in use at Holyhead and Southport, and they had been found to agree fairly well in their maxima with the non-oscillating plates. There were a very large number of records now available from these instruments, and so far as he knew only once had a pressure-tube anemometer recorded a pressure exceeding 30 lbs. per square foot.

Mr. Fergusson. Mr. S. P. Fergusson, of Blue Hill Observatory, U.S.A., congratulated the Author on having made a valuable contribution to knowledge of the difficult subject of wind-pressure. As the devices for measuring or comparing velocities and pressures had been improved, the pressure-velocity constant had become smaller and

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[1] Quarterly Journal of the Royal Meteorological Society, vol. xxv (1899), p. 9.
[2] *Ibid.*, vol. xx, p. 180.

smaller, until, in the value obtained by the Author in a uniform current, there was a close approximation to the true theoretical formula proposed by Professor Ferrel[1]— Mr. Fergusson.

$$p = \frac{0 \cdot 002698 \, V^2}{1 + 0 \cdot 004 \, t} \, \frac{P}{P_0}$$

in which p = pressure of the wind in pounds per square foot,
P_0 = the standard barometric pressure (760 millimetres),
P = the barometric pressure at the station of observation,
V = the velocity of the wind in miles per hour,
and t = the temperature in degrees Centigrade.

For an average temperature, say 15° C., and air of standard pressure 760 millimetres, this formula became:—

$$p = 0 \cdot 00255 \, V^2.$$

The variations of the mean and extreme intensities of wind-pressure on large and small plates noted by the Author were confirmed by Mr. Fergusson's own experiments with large and small anemometers at Blue Hill Observatory in 1892–94. In some tests of the sensitiveness of several anemometers of different sizes it was found that each instrument would adapt itself to a sudden change of velocity during one or two rotations of the cups or fans, irrespective of the size of the anemometer. Hence, the smaller the anemometer, the more readily it followed extreme and rapid fluctuations in velocity. The large instruments recorded an approximation to the mean velocity of the wind. If it were possible to eliminate the effects of inertia and friction he did not doubt that the value of K would be the same in the natural variable wind as it was in a steady current. The accuracy of velocity- as well as pressure-anemometers, and of wind-vanes, was more or less affected by inertia, and the sensitiveness of the velocity-anemometer should be as nearly as possible the same as that of the pressure-anemometer when comparisons were to be made. This remark applied also to methods of recording. The Author's method of eliminating the effects of inertia of the plates was admirable, and if his experiments were to be repeated Mr. Fergusson would suggest but one change—that the plates and velocity-anemometers should be made to record automatically upon a rapidly-moving chronograph drum, thereby avoiding any errors due to the personal equations of the observers.

Mr. F. Hudleston observed that the Author's "General Conclusions" did not seem quite consistent. On p. 200 the Author stated that there appeared to be no reason to doubt the correctness Mr. Hudleston.

[1] W. Ferrel, "A Popular Treatise on the Winds," p. 372. London, 1889.

Mr. Hudleston. of his coefficient 0·0032 in the formula $P = KV^2$ for the strongest gales, as well as for the somewhat moderate breezes from which he deduced that value; yet on p. 199 he expressed the opinion that the Bidston Observatory records of the 9th March, 1871, and the 23rd November, 1877, might be not far wrong. Now these records, as quoted by the Author, required a coefficient of not less than 0·0144 and 0·010 respectively. It appeared to Mr. Hudleston that most people who discussed the Bidston records took it for granted that the velocities quoted referred to the identical moment of time when the high pressure was recorded, whereas they actually gave the mean velocity of the whole hour in which the high pressure occurred. An examination of the Bidston records of the 9th March, 1871 (which were published in the Report of 1881), showed that, immediately before the Osler instrument registered 90 lbs. per square foot, the Robinson cup instrument had recorded 16 miles of wind passing in 0·12 of an hour, or, in other words, had recorded a velocity of 133 miles an hour for 7 consecutive minutes; a very different velocity to the 79 miles usually quoted in connection with this particular record, and one which would only require K to equal 0·005 in order to satisfy the formula $P = KV^2$; indeed it could easily be conceived that a few seconds of those 7 minutes might have satisfied the Author's coefficient. The Bidston instruments had been frequently watched during heavy gales by the late Mr. Hartnup, Jun., Astronomer to the Mersey Docks and Harbour Board, who had charge of these instruments, and he had personally told Mr. Hudleston many years ago that the movements of the Osler indicating needle when recording high pressures were generally very deliberate and did not support the view so frequently expressed that the momentum of the plate or other moving parts carried the pencil far beyond the position due to the static pressure on the plate. As regarded the relation of local pressure to that on a large surface Mr. Hudleston's view was that a heavy gale had in it many eddy currents—any man in a gale could feel himself being buffeted from side to side—and a small pressure-plate on what might be termed the leading side of such an eddy must of necessity register in excess of the mean pressure over a large area; and in his opinion the Forth Bridge tests corroborated this view.

Prof. Kernot. Professor W. C. Kernot hailed with satisfaction any attempt to put the vexed subject of wind-pressure on engineering structures on a sounder foundation. Twenty-five years ago he was interested in a bridge that was condemned as dangerous under wind-pressure, although a legitimate calculation showed that it required 90 lbs. per square foot over the total area of both girders, as well as over the piers, to overturn it; whereas in the same locality, but in more exposed

Prof. Kernot.

positions, domestic chimneys and railway-carriages that would overturn with 35 lbs. per square foot abounded, and had proved perfectly safe. Doubtless the condemnation and costly reconstruction of this bridge was due to the scare arising from the Tay Bridge disaster, which occurred about 3 years before. This incident led him to take special interest in the question of wind-pressure, and with the help of his assistant, Mr. James Mann, he made a series of experiments similar to those of the Author, but with less perfect appliances, limited leisure and limited funds compelling the use of roughly extemporized machinery. In spite of this, however, the results were definite and consistent, and checked on repetition to within 2 or 3 per cent. and often closer. As the Melbourne Observatory was provided with a velocity-anemometer only, it was necessary to establish a pressure-velocity formula. That given in the text-books accessible was $P = KV^2$, P being the pressure in pounds per square foot, V the velocity in miles per hour, and K a coefficient which was given on the authority of Smeaton and Rouse as 0·005. After various unsuccessful attempts with other apparatus a fairly consistent set of results was obtained with a whirling-machine having a rectangular pressure-plate 8 inches by 4½ inches, at speeds varying from 12 to 27 miles per hour: from these a value of 0·0033 was obtained for K. It was satisfactory to note how well this had been verified by all recent tests, the Author's included. These experiments, it should be added, were conducted at a spot about 150 feet above sea-level, at a temperature of 60° to 70° F., and a barometric pressure between 29·5 and 30 inches. How the earlier experimenters came to exaggerate the pressure 50 per cent. it would be interesting to know. A second set of experiments was made with a blowing-machine giving a jet of air 12 inches square as against the Author's 24 inches. In this were placed small models of towers, chimneys, roofs, girders, etc., and the pressure on them was measured by comparison with flat plates of equal projected area. The results agreed fairly well with the Author's. The experiments occupied several years and the results had been published.[1] The most surprising result of practical importance arrived at was the extraordinary difference in the pressure on a roof supported upon columns, such as those of the Melbourne wharves, and on a similar roof upon walls. In the former case the well-known formulas of Hutton and Duchemin[2]

[1] Reports of the Australian Association for the Advancement of Science, vol. v (1893), p. 573, and vol. vi (1895), p. 741.

[2] See W. H. Warren, "Engineering Construction in Iron, Steel and Timber," p. 286.

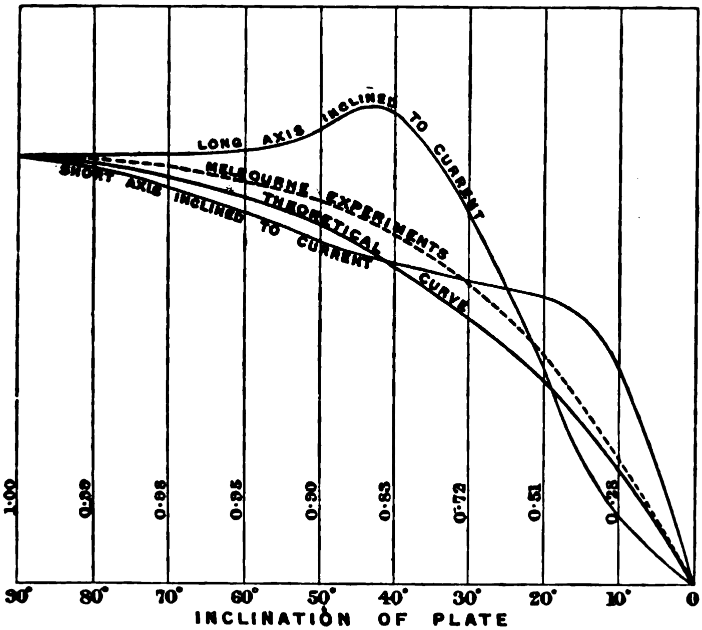
Prof. Kernot. were practically confirmed; but in the latter case the existence of a "wind-shadow" was determined, within which there was little or no pressure. The boundary of this wind-shadow was explored with a little flag of silk fibre at the end of a wire, and was very definite. In a building of ordinary proportions with a roof of 30° pitch the pressure was so small as to be nearly negligible, while with a pitch of 45° it was less than one-third of the figure given by Hutton's formula, and this was so even if the building had large openings on the leeward side. Further, the presence of parapets of ordinary proportions raised the "wind-shadow" so high that even with 60° pitch the pressure was less than one-third of that usually assumed. This marked reduction of wind-pressure on ordinary roofs was confirmed by the Author's earlier experiments, and was of the greatest importance in practice, enabling roof-construction to be made more economical. As so many roofs were provided with parapets, it would be a good thing for the Author to extend his experiments to this case, and see if his results confirmed those obtained in Melbourne. The principal results of the Melbourne experiments were here appended. They of course dealt only with the total pressure, no attempt having been made to determine its distribution over the surface, or what portion was positive pressure on the front and negative pressure on the back of the object.

Summary of the more Important Results of Experiments on Wind-Pressure at the Melbourne University during the Years 1891–94.

Value of K in pressure-velocity formula	0·0033
Relative pressure per square foot on objects of various forms calculated for the cross section of greatest area normal to the direction of the wind :—	
(*a*) Thin plate of square form	1·00
(*b*) Cube, one face normal to wind	0·90
(*c*) Ditto, one face diagonal to wind	0·63
(*d*) Square tower, height three times base, one face normal to wind	0·90
(*e*) Ditto, diagonal normal to wind	0·63
(*f*) Square pyramid height three times base, one side normal to wind	0·80
(*g*) Ditto, one diagonal normal to wind	0·70
(*h*) Cone, height three times base	0·50
(*i*) Cylindrical tower	0·52
(*j*) Octagonal ,, ...	0·60
(*k*) Sphere	0·36
(*l*) Hemispherical cup, convex to wind	0·36
(*m*) Ditto, concave to wind	1·15
(*n*) Lattice girder of ordinary type	1·45
(*o*) Plate-girder bridge with top or bottom deck, width between girders equal to depth	1·00
(*p*) Ditto, width between girders twice depth . . .	1·20

A series of experiments on the effect of wind on a thin plate, the length being about one-and-a-half times the width, and the longer axis at right angles to the direction of the wind, gave results approaching Lord Rayleigh's theoretical curve,[1] as shown by the dotted line and figured ordinates in *Fig. 18*. As the proportions of Prof. Kernot.

Fig. 18.



the plate used in Melbourne were intermediate to those in the Author's experiments, so the resultant curve was for nearly its whole course intermediate to his two curves, shown here in full lines.

Mr. A. Mallock remarked that in this Paper the Author arrived at a more normal value for the coefficient of resistance than that which he obtained from his small-scale experiments. He gathered that the Author was inclined to impute this difference to a real dimensional effect. In this he could not agree, as it was difficult to imagine that the physical properties of a gas could involve an absolute length of some inches. This was what must be supposed if there was a real change in the coefficient of resistance as the dimensions of the resisting surface changed from a few inches Mr. Mallock.

[1] Minutes of Proceedings Inst. C.E., vol. clvi, p. 102, *Fig. 15*.

Mr. Mallock. to a few feet. If there was such a change it must be related either to viscosity or surface friction. The difference between the resistance of a plate and of a lattice girder of the same total area might be expected from the fact that with the plate the flow was three-dimensional, and with the girder approximately in two dimensions. The experiments appeared to have been made with great care, and were, he thought, likely to be of much practical value. He did not, however, look on open-air experiments as the best means of determining the numerical value of the resistance-coefficient. The wind near the surface of the ground was full of eddies, whose axes might be inclined at any angle to the horizontal, and whose transverse dimensions might be anything from a few feet upwards. Such eddies, of course, made themselves felt as variations in velocity and direction, and caused difficulties and doubtful points to arise which would be avoided by propelling the surface at a known speed through still air.

Mr. Shelford. Mr. Frederic Shelford considered that the practical results of the experiments described in the Paper were somewhat limited. Few engineers were interested in winds of velocities less than 30 miles per hour. An engineer responsible for building a large structure really wished to know the highest velocity of wind likely to occur at the site. The Author naturally must leave the determination of this velocity to the engineer himself, but it would have been interesting if the Author had inquired into the highest wind-velocities recorded. The engineer having determined for himself the greatest velocity of wind with which he might have to deal, the Author's experiments were intended to assist in determining the intensity of pressure due to that velocity. But this could be done only by extending the diagrams obtained by the Author's experiments from 30 miles per hour to the 70 to 80 miles per hour for which the engineer might decide to allow. The assumption that the relation of velocity and pressure remained constant at higher values was unconvincing. It should be possible to add to the value of the Author's experiments by carrying out tests at higher velocities, and he believed that good results might be obtained from some experiments he had been making on wind-pressure at the high velocities attained by a racing motor-car at the Brooklands motor-track. He had recently fitted to a 120-HP. racing-car, kindly lent to him for the purpose by Messrs. de Wilton and Blomfield, a pressure-recording apparatus of the simplest description. It consisted of a circular screen 18 inches in diameter held in front of the frame of the motor-car and carried by a piston-rod arrangement. The pressure of the air upon the screen was transmitted over a pulley to a spring-balance.

Mr. Shelford.

The speed of the car, and therefore, on a calm day, of the artificial wind created by its motion, had been recorded with great accuracy by the electric timing-apparatus at Brooklands, by the kindness of the authorities. This method of observing wind-pressures in a wind artificially created had considerable advantages. Any wind from 5 to 90 or even 100 miles per hour could be obtained at will, and the actual pressure on the screen could be observed with little possibility of error, although not with any great refinement. Such a wind was of course free from gusts or squalls; the pressure was exactly normal to the face of the screen, there was no inertia-effect of the apparatus, and the oscillations of the car at high speeds only served to overcome any slight friction of the piston-rod bearings. The difficulty of observing the gauge at high speed was overcome by a recorder fixed to it. It seemed to him that such an apparatus would allow observations of wind-pressure to be made, beyond all doubt as to accuracy, up to velocities never attained by natural winds. Further, the relative pressures upon plates of various sizes and shapes could be rapidly determined. There would be no difficulty in carrying a model of a girder or bridge upon the apparatus and measuring the air-pressure upon it at any desired velocity. He regretted that he was not yet in a position to give results of his experiments. In the experiments made so far the speed could not exceed 60 miles per hour owing to the insufficient strength of the gauge used, which was pulled out to its maximum at a higher speed than this. He hoped to communicate definite results at a later date.

Mr. Wingfield.

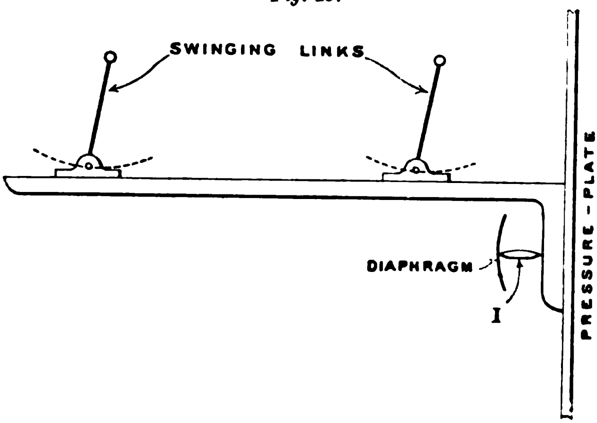
Mr. C. Humphrey Wingfield asked whether the ribs mentioned on p. 184 were of the same depth (1½ inch) on both large and small plates. If so, would they not interfere somewhat with the comparison between large and small but otherwise similar plates? He noticed, lower down on the same page, that the Author had placed the static tube at right angles to the position described in his former Paper. This tube did not seem to be a very satisfactory arrangement, as the Author had noticed[1] that appreciable difference in the readings resulted from a slight alteration of its position relatively to the air-current. Mr. Wingfield again suggested that some form of mouthpiece for the static tube should be sought for which would be independent of the direction of the air-current. Had the Author tried that to which he had previously called attention?[2] This form appeared to be quite unaffected by the direction of air-currents when it was properly constructed. He was not certain that he understood the

[1] Minutes of Proceedings Inst. C.E., vol. clvi, p. 138, last line.

[2] *Ibid*, p. 136.

Mr. Wingfield. construction of the knife-edged supports referred to on p. 183. Did they allow the part of the plate to which they were attached to move freely to and fro, in a direction parallel to the air-current? If not, a correct reading could be obtained only when the centre of wind-pressure happened to coincide with the centre of the pin. The evidence of the irregularity of wind-pressure brought forward by the Author suggested that this would happen only occasionally. Except when it did so, the knife-edges would take a part of the pressure which was supposed to come on the pin. Moreover, if, as would appear from Fig. 10, Plate 3, the plate rocked on the knife-edges as if they were hinges, whenever the momentary centre of pressure happened to be above the pin, the latter would register a load in excess of that really due to the average pressure. Mr.

Fig. 19.



Wingfield suggested supporting the plates by means of two horizontal bars, each carried by two links, so as to permit a motion similar to that of an ordinary parallel ruler (*Fig. 19*). The plate would always then remain "parallel to itself," and the readings would not be subject to the possibility of serious error which he had pointed out. Possibly a better arrangement would be obtained by regarding the Figure as upside down. If the links were attached to shafts passing under the platform the latter would confer rigidity without getting in the way of the observer.

The Author. The AUTHOR, in reply, remarked that considerable criticism had been aroused by his statements with reference to the Bidston Observatory records. This, he thought, was due to a misunderstanding; for the interpretation of these records which he considered

The Author.

unjust was in no way concerned with their magnitude, but with the common assumption that, because the pressure-record per square foot for a given velocity-record varied within wide limits, such records were manifestly inaccurate. This he had shown was not the case. He fully realized that such records of the hourly run of the Robinson cups and the maximum pressure in that hour were quite valueless regarded as an estimate of the pressure due to a given velocity of wind The large coefficients which Mr. Hudleston had derived from the Bidston records had nothing to do with the actual velocity at the instant of maximum pressure, so that no comparison of these values with the Author's coefficient was possible. Whether or not pressures of such magnitudes, even on small plates, had ever existed in England, might well be doubted; but it might be pointed out that the Wind-Pressure Commission of 1881, of which the late Sir George Stokes was a member, expressly stated that their estimate of 56 lbs. per square foot was based on records of pressures which, they were satisfied, were not due to instrumental error depending upon momentum, but which represented a real phenomenon.[1] That such errors did exist, however, had been clearly shown by the experiments of Mr. Dines in 1894. The explanation of the difference in resistance of large and small plates, referred to by Mr. Mallock, had been dealt with in the Author's remarks at the commencement of the discussion. Since the reading and discussion of the Paper the results of Mr. Eiffel's experiments, made on plates let fall from the second stage of the Eiffel tower, had been published and they were in close agreement[2] with those obtained at the National Physical Laboratory on large plates in the wind and on small plates in a uniform current of air. The objection raised by Mr. Shelford to the assumption that the pressure-velocity relation remained constant up to velocities of 70 or 80 miles per hour was also fully met by the results of Mr. Eiffel's experiments, in which this relation was found to be practically constant up to 90 miles per hour. In reply to Mr. Wingfield's questions, the ribs of the plates varied with the dimensions, except in the case of the 50-square-feet and 100-square-feet boards, where for constructional reasons they were the same. For wind-experiments the Author had found the form of suction tube used by Mr. Dines so satisfactory that he had not tried any other form. He agreed with the criticism of

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[1] "Report of the Committee appointed to consider the question of Wind Pressure on Railway Structures," p. 1. London, 1881.

[2] See T. E. Stanton, "The Air-Resistance of Plates." *Engineering*, vol. lxxxv (1908), p. 605.

The Author. Mr. Wingfield on the knife-edge arrangement adopted, and thought that the method of support of which he had given a diagram was admirable. In reply to Major Baden-Powell's question, no estimation of the deviation of the wind from the horizontal direction had been made, but from the nature of the ground in front of the tower the Author was of opinion that such deviation was inappreciable. In conclusion, the Author desired to thank Mr. Dines for his valuable contribution to the discussion, with the whole of which he was in complete agreement.

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10 December, 1907.

Sir WILLIAM MATTHEWS, K.C.M.G., President, in the Chair.

(*Paper No. 3686.*)

"The Predetermination of Train-Resistance."

By CHARLES ASHLEY CARUS-WILSON, M.A., Assoc. M. Inst. C.E.

THE RESISTANCE-EQUATION.

THE elements of train-resistance on a straight and level track may be summarized as: —

1. Journal-friction, due to the rubbing of the journals on the brasses.
2. Rolling-friction, due to the rolling action that takes place between the tread of the wheel and the rail.
3. Track-resistance, due to the compression of the track as the train advances.
4. Flange-action, occasioned by the side pressure of the flanges on the rails.
5. Air-resistance, caused by the force of the air on exposed parts.

Journal-Friction.—The horizontal effort required to overcome the friction of a journal increases directly as the diameter of the journal and inversely as that of the wheel. It may be expressed by the formula

$$2{,}240 \frac{a}{d} \mu \text{ lbs. per ton} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where a and d are the diameters of the journal and wheel respectively, in inches, and μ is the coefficient of friction.

Numerous tests have been made to ascertain the value of the coefficient of friction.[1] With perfect lubrication, as with an oil-

[1] See B. Tower, "First Report on Friction Experiments," Proceedings Inst. Mechanical Engineers, 1883, p. 632: also J. Goodman, "Recent Researches in Friction," Minutes of Proceedings Inst. C.E., vol. lxxxv (1886), p. 376, and Proc. Manchester Assoc. Engineers, 1890, p. 87.

bath, the coefficient varies inversely as the pressure on the journal, but with lubrication of an intermediate character, such as that commonly obtaining with railway-carriage axles, the coefficient remains nearly constant with varying pressures.

The following are given by Mr. Beauchamp Tower as the results of tests made by him on a 4-inch journal lubricated with rape-oil by a pad under the journal. The pressures are those of the total load divided by the projected area of the brass. The speed is given by Mr. Tower at 200 revolutions per minute, which is equivalent to a speed of 25 miles per hour on a 42-inch wheel.

Load, lbs. per square inch	178	272	364	458	498	520	551	582
Temperature, ° F. . .	75	74	82	78	77	76	82	90
Coefficient of friction .	0·0109	0·0096	0·0087	0·0095	0·0091	0·0105	0·0099	0·0107

Mr. W. Stroudley experimented with a journal 3½ inches in diameter, lubricated with a pad of "Globe" oil, running under conditions similar to those of carriage-axles on the London, Brighton and South Coast Railway. The speed of the journal was kept constant at 390 revolutions per minute, corresponding to a speed of 49 miles per hour on a 42-inch wheel. The following were the results obtained:

Load, lbs. per square inch	237	251	293	333
Coefficient of friction .	0·00720	0·00770	0·00800	0·00792

Under similar conditions it is found that the coefficient of friction changes but little with variations in the speed. The following figures represent the results of tests made by Mr. Tower at a pressure of 364 lbs. per square inch, and a temperature of 82° F. The speed is reckoned as before by assuming a 42-inch wheel.

Speed, miles per hour .	18·7	25·0	31·2	37·5	43·7	50·0
Coefficient of friction .	0·0105	0·0087	0·0085	0·0078	0·0085	0·0100

From these tests it appears that under the conditions usual in ordinary railway practice, journal-friction is independent both of speed and load, and may be taken to be a constant quantity depending only upon the wheel- and journal-diameters. Taking the mean of the values obtained by Mr. Stroudley, namely, 0·0077 for 280 lbs. per square inch, the friction for a 4-inch journal with a 42-inch wheel would be 1·65 lb. per ton.

Rolling-Friction.—The resistance to the motion of a wheel rolling on a clean smooth rail arises from the elastic indentation of the rail and the consequent friction as the rail rubs over the surface of the wheel in its endeavour to regain its normal level. This action has been fully explained[1] by Professor Osborne Reynolds, F.R.S. The rolling-resistance is independent of the speed, and varies inversely as the wheel-diameter; it may be expressed by the formula

$$2{,}240 \ \frac{f}{d} \text{ lbs. per ton} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where f is a constant depending upon the nature of the surfaces, and d is the diameter of the wheel in inches.

No direct tests of any practical value seem to have been made to ascertain the value of f for railway-wheels. References to such tests as have been made are given by Weisbach.[2] For steel rollers of $\frac{3}{4}$ inch diameter rolling at 8·6 miles per hour on a smooth steel surface with a load of 1,180 lbs. per lineal inch the value of f has been found to be 0·0022, and to decrease as the speed and pressure increase. Using this value in the formula, the rolling-friction of a 42-inch wheel would be 0·118 lb. per ton for the above speed and pressure. The pressure of a railway-wheel on a rail is much greater than 1,180 lbs. per lineal inch, so that the rolling-friction of a wheel on a rail at normal speeds is probably less than the value found above.

Track-Resistance.—The leading wheels of a train depress the track, and work is thus done to provide for which a definite pull has to be exerted. If the track completely recovers its normal condition before a second axle reaches the same point, the pull is given by

$$P = 2{,}240 \ \frac{h}{2\,k} \text{ lbs. per ton} \quad . \quad . \quad . \quad . \quad (3)$$

where the ratio of h to k is the inclination of the rail produced by the depression under the axle (See Appendix, Note 1). Complete recovery is attained only at very low speeds; as the speed increases the period of recovery is cut short by the succession of wheels at any one point, and the conditions rapidly approximate to those of

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[1] "On Rolling-Friction." Philosophical Transactions of the Royal Society, vol. 166 (1887), p. 155. See also J. H. Cotterill, "Applied Mechanics," 5th ed., p. 250. London, 1900.

[2] J. Weisbach, "A Manual of the Mechanics of Engineering, etc.," vol. i, p. 354. London, 1877.

no recovery until the whole train has passed. The pull in pounds per ton given above has then to be divided by N, the number of axles in the train.

The inclination of the rail depends upon the character of the track. If the track is light, the depression produced is local and the inclination considerable; but if the track is stiff, the load is distributed over a length of rail and the inclination is reduced. Mr. J. A. F. Aspinall has given the inclination as observed by himself at 1 in 580: Mr. A. Mallock has observed inclinations of 1 in 250 and 1 in 400. Pending more complete observations, the value of the inclination of a first-class track under a loaded axle will be taken to be 1 in 400.

When a train is just moving, track-resistance is then 2·8 lbs. per ton; as the speed increases this resistance will diminish until the depression becomes permanent, when the resistance will be the above amount divided by the number of axles in the train. Observations are needed to determine precisely the speed at which the depression becomes permanent; the Author has been unable to obtain indications of any recovery at 10 miles per hour, and it would seem that above this speed the depression is wholly permanent. For a single bogie-coach with four axles the track-resistance would then be 0·7 lb. per ton, and would be independent of the speed. For a train, say, of five such coaches track-resistance would be 0·14 lb. per ton, a neglible quantity. Hence it would appear that for trains running at more than 10 miles per hour track-resistance may be neglected except in the case of single coaches, or motor-coaches with one or two trailers.

Flange-Action.—Owing to the necessary clearance between the flanges and the rails the carriages or wagons making up a train are continually moving at a slight angle to the rail, causing a loss of energy in friction. With a rigid wheel-base the whole weight of the wagon and its load is subject to this flange-action, whereas, with a bogie-wagon, only the weight of the bogies is involved, the body of the wagon, with the load, remaining more or less in the centre of the track, or subject only to a slow and gradual side sway. The resistance to motion thus caused is proportional to the first power of the speed; it depends also upon the play between the flanges and the rails, and varies inversely as the wheel-base (See Appendix, Note 2). For a carriage with a rigid wheel-base b, moving with a velocity v, the resistance to motion is thus proportional to

$$\frac{v\,c}{b} \text{ lbs. per ton} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where c is the total play between the flanges and the rails. For a bogie-wagon the resistance is given by

$$\frac{w}{W}\frac{v\,c}{b} \text{ lbs. per ton} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where w is the weight of the two bogie-trucks, and W is the weight of the whole car, including the trucks.

Air-Resistance.—The resistance due to the air encountered by a moving train increases as the square of the speed, and is made up of four distinct elements, namely, front pressure, rear suction, side and top friction, and underneath friction.

The force on a small flat plane with an area of ω square feet moving at right angles to its surface through still air at V miles per hour is given by the equation

$$P = 0 \cdot 00254 \ \omega \ V^2 \text{ lbs.} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

At 60 miles per hour the force would thus be 9·14 lbs. per square foot of projected area. The force on the front and rear of a train or car is, however, considerably less than this.

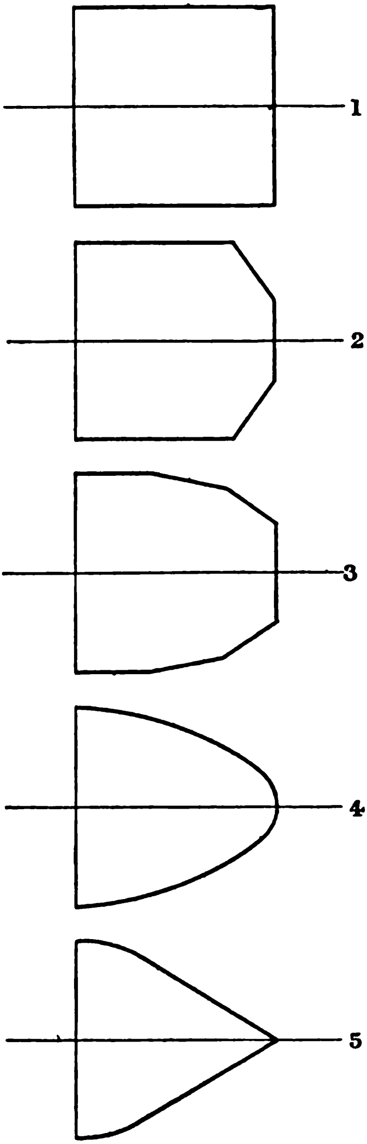
The most complete tests hitherto made to ascertain the amount of this force at various speeds are those conducted by the Electric Railway Test Commission at St. Louis in 1904, a full account of which may be found in the Report of the Commission.[1] In these tests a special car was constructed with arrangements by which the force of the air on the front and rear of the car could be measured. Vestibules with various profiles were fitted to the car, and the force on each was observed. The profiles experimented with are shown in outline in *Figs. 1*, namely, No. 1, Flat; No. 2, Standard; No. 4, Parabolic; and No. 5, Parabolic wedge. (No. 3 profile will be referred to later.) The observed resistance of the air at 60 miles per hour in pounds per square foot of projected area is given in the following Table:—

TABLE I.

	Vestibule in Front. Pressure.	Vestibule in Rear. Suction.	Total.	Ratio to that on a Flat Plane.
	Lbs. per Sq. Ft.	Lbs. per Sq. Ft.	Lbs. per Sq. Ft.	Per Cent.
No. 1. Flat	8·20	0·50	8·70	95
„ 2. Standard . . .	4·53	1·40	5·93	65
„ 4. Parabolic . . .	2·50	0·24	2·74	30
„ 5. Parabolic wedge .	2·10	0·45	2·55	28

[1] New York, 1906.

Figs. 1.



It will be seen that whereas the combined front and rear forces on a flat-ended car are 5 per cent. less than the theoretical force on a flat plane, by shaping the ends a considerable reduction can be made, the force on the front and rear of a vestibule of standard profile showing a reduction of 35 per cent., while with parabolic and parabolic wedge profiles the reductions are 70 and 72 per cent. respectively. It is evident that in any attempts to predetermine train-resistance, the shape of the ends of the train must be taken account of. In what follows the values of the resistance obtained by the St. Louis Commission as given in the Table will be taken. The resistance at other speeds may be deduced from those at 60 miles per hour.

If there is a head wind of V miles per hour, the resulting force will be obtained by increasing the speed of the train in the resistance-equation relatively to the air by that amount. The effect of a side wind blowing at right angles to the length of the train is to press the flanges of the wheels against the rails and so to cause friction. The amount of the pressure will depend upon the side area exposed and the force per square foot exerted by the wind. Experiments are needed to show what is the amount of this force, but it may be taken to be certainly not less than that of the combined pressure and suction at the front and rear of a train with flat

ends, and in this Paper it will be assumed to be 8·7 lbs. per square foot of exposed side area. The tests made by Professor J. N. Franke[1] indicate that the coefficient of friction between the wheel and the rail will be about 0·3, which value will be taken in this Paper.

The resistance due to the friction of the air on the sides and the top of a train depends upon the roughness of the exposed surface. The coefficient of friction for air against a flat surface is known to lie between 0·0025 and 0·0044. Some tests recently made by Mr. B. C. Batcheller in the pneumatic-despatch tubes of New York make this coefficient to be 0·0032 for a machined cast-iron surface. Assuming this value, the frictional resistance would amount to 30 lbs. per 1,000 square feet of exposed area at 60 miles per hour. In view of the irregularities due to the exterior fittings with which the outside of a railway-carriage is covered, the actual resistance must be much in excess of this figure. How great an influence such roughnesses may exert can be gathered from Mr. Froude's experiments on the friction of water on different surfaces, in which he found that the frictional resistance of water against a board covered with coarse sand was twice as much as that against a similar board covered with tin-foil. Experiments were therefore needed to determine the friction of the air against the sides and top of a train.

The air-friction underneath a train is caused by the obstruction offered to the free passage of the air under the carriages. This will depend largely upon whether bogies are used, and it will be assumed that these offer a resistance to the air compared with which that due to individual wheels and axles may be neglected. Nothing definite is known as to the amount of this underneath friction, which must therefore be determined by actual test.

The component parts of train-resistance may thus be divided into three groups as follows:—

1. Those resistances which are independent of the speed. These comprise journal-friction, rolling-friction, and track-resistance.

2. Those resistances which vary with the first power of the speed. These are the resistances caused by flange-action.

3. Those resistances which increase with the square of the speed. These include all forms of air-resistance.

The general equation for the whole resistance will then be of the form:

$$R = A + Bv + Cv^2 \quad . \quad . \quad . \quad . \quad . \quad (7)$$

In this equation A is only partially known—that is, journal-friction

[1] "Ueber die Abhängigkeit der gleitenden Reibung von der Geschwindigkeit." *Der Civilingenieur*, 1882, p. 205. Abstract in Minutes of Proceedings Inst. C.E., vol. lxix, p. 461.

has already been ascertained with tolerable accuracy, but rolling-friction and track-resistance have not been actually determined by test, though they are probably small compared with journal-friction. B is hitherto quite unknown. The only part of C that is known is the front pressure and rear suction.

Determination of the Constants in the Resistance-Equation.

The constants in the equation for train-resistance must be derived from the results of tests made under actual working-conditions. There is great difficulty in making a suitable selection of tests for this purpose, partly on account of the errors to which train-resistance tests are liable and partly because in hardly a single case has a record been kept of all the data upon which train-resistance depends. In the majority of cases it has been considered sufficient simply to state the type of coach or wagon used, and then give the results of the test, but with no record of the dimensions of the carriages, of the wheel-base, of the weights of the trucks or of the journal- or wheel-diameters.

A greater difficulty has been the scarcity of tests whose results may be accepted as free from serious error. Most of the recorded tests have been made with a dynamometer, and such tests are peculiarly liable to error on account of the corrections for retardation and acceleration that have to be introduced even when run on straight and level lines. It is difficult to observe the speed with less than 1 per cent. of possible error. If a train is running at 30 miles per hour, and speed observations are being taken at every half-minute, a variation of 1 per cent. in the speed would mean a variation of 0·014 foot per second per second—that is, an accelerating or retarding force, as the case might be, of about 1 lb. per ton. If the total resistance at this speed is, say, 10 lbs. per ton, an error in the speed of 1 per cent. may thus involve an error of 10 per cent. in the resistance. The only way of meeting this objection is to make a large number of observations with as much accuracy as possible, plot the results, and deduce a curve giving the mean.

A more reliable method is by retardation, where the train is allowed to coast on its own momentum and observations are made of the retardation at different speeds. In consequence of the tendency of the vehicles of a train to crowd together when coasting, this method is best adapted for testing a single coach, and it was in

this way that the resistance of the high-speed electric cars was determined in the Zossen tests.

Perhaps the most satisfactory method where practicable is to run the train over a length of 100 miles or more of line, taking autographic speed- and dynamometer-records from which may be deduced the mean speed and the total work done on the train. After correcting for the difference between the levels at start and finish, a close approximation can be made to the mean draw-bar pull per ton of load, especially if the line be fairly straight and the run continuous.

After a careful investigation of the different tests available, the Author selected for the determination of the constants in the resistance-equation the tests made by Mr. Barbier on the Northern Railway of France.[1] These tests were made with a dynamometer-car and extended over a period of more than 4 years (1891–95), and the only results considered were those obtained at virtually constant speeds on straight lines having a uniform profile. The tests took place in all seasons, and under the varying conditions of actual service, so that the results obtained correspond to mean atmospheric conditions and to the normal state of the track. The final results are expressed by curves which are the outcome of a large number of tests on the same train, and relate to two classes of rolling stock. Thanks to the courtesy of Mr. Barbier the Author has been able to secure all the data required to interpret the results.

The trains tested were made up of two kinds of vehicles, one set of trains consisting of the ordinary first- and second-class four-wheeled passenger-coaches of the Northern Railway, and the other of bogie-coaches owned by the *Compagnie Internationale des Wagon-Lits.* The curves by which Mr. Barbier represents his final results are reproduced in Fig. 2, Plate 4, the upper curve giving the resistance of the four-wheeled coaches and the lower that of the bogie-coaches, in pounds per ton, at different speeds.

The wheels and axles were similar in the two sets of trains, the journals being 4·1 inches and the wheels 41 inches in diameter. Hence the journal- and rolling-friction was the same.

The tractive effort was observed on a dynamometer-car placed behind the locomotive: hence the track-resistance need not be considered, since it is provided for by the locomotive. The quantity A in the general equation for train-resistance is therefore the same in the two sets of tests.

An inspection of the two curves shows that the resistance of the four-wheeled trains is greater than that of the bogie-coaches, and that

[1] "Résistance à la traction des trains de voyageurs à grande vitesse en alignement droit." *Revue générale des Chemins de Fer*, vol. xx (1897), Pt. I, p. 272.

the difference is throughout proportional to the speed. Thus, at 30 miles per hour the difference is 2 lbs. per ton, while at 60 miles it is 4 lbs. Since both curves have equations of the form $A + Bv + Cv^2$, and the difference is proportional to v, it follows that the terms A and Cv^2 are the same in each, and that the difference is due to that element of the resistance which varies as the first power of the speed, that is, to the different amounts of flange-action in the two classes of rolling stock. It has already been shown that A must be the same in each. The fact that the air-resistance in the two trains is the same is of course a mere coincidence.

The play between the flanges and the rails was 28 millimetres (1·1 inch) throughout, hence the ratio of the flange-action with the four-wheeled coaches to that with the bogies is given by the expression:—

$$\frac{\text{bogie-wheel base}}{\text{four-wheel base}} \times \frac{\text{weight of whole bogie-car}}{\text{weight of two bogie-trucks}} \quad . \quad (8)$$

The wheel-base of the four-wheeled cars was 17·7 feet, that of the bogies 8·2 feet. The weight of the whole bogie-car was 33·8 tons, that of the two bogie-trucks 10·0 tons. Hence the ratio of the flange-actions is 1·57.

Taking the difference as given by the curves at 60 miles per hour, namely, 4 lbs. per ton, and knowing the ratio to be 1·57, the flange-action of the bogie-carriages at this speed is found to be 7·0 lbs. per ton, and that of the four-wheeled cars 11·0 lbs.

The remaining elements of the resistance can now be determined. The total resistance for the bogie-coaches at 60 miles per hour is 14·3 lbs., therefore air- plus journal- and rolling-friction is 7·3 lbs. At any other speed, say 30 miles an hour, the total is 6·5, the flange-resistance 3·5, and air- plus journal- and rolling-friction therefore 3·0 lbs. per ton. Now the journal- and rolling-friction is the same at both speeds, hence the difference between the air-resistance at 60 and that at 30 miles per hour is 4·3 lbs., while the former is four times the latter, hence the air-resistance at 60 miles per hour is 5·7 lbs. per ton. Journal- and rolling-friction is 14·3 lbs. less air- and flange-resistance, that is, 1·6 lb. per ton.

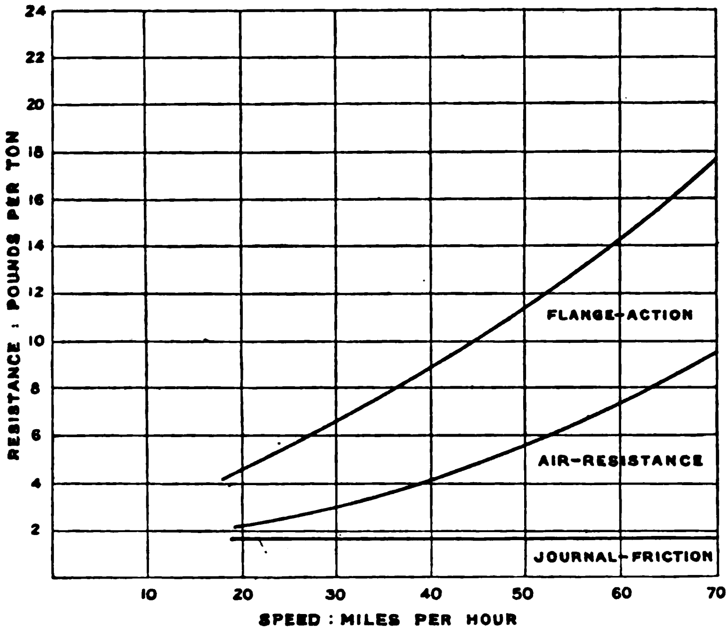
The total resistance of the two kinds of rolling stock at 60 miles per hour is therefore made up thus:—

TABLE II.

	BOGIES. Lbs per Ton.	FOUR-WHEELED COACHES. Lbs. per Ton.
Journal- and rolling-friction	1·6	1·6
Flange-action	7·0	11·0
Air-resistance	5·7	5·7
Total . .	14·3	18·3

The component parts of the total resistance for any other speed may be deduced from those for 60 miles per hour, the journal- and rolling-friction remaining constant, the flange-action varying as the speed, and the air-resistance as the square of the speed. This has been done for the bogie-coaches, and the results are plotted in *Fig. 3*. These curves are applicable only to the particular trains on which the tests were made. If a train is made up of coaches differing in any way from those tested, allowance must be made for the changed conditions in accordance with the following formulas:—

Fig. 3.



Journal-Friction.—The journal- and rolling-friction are very nearly the same as that calculated previously, and it seems clear that for a smooth clean rail the rolling-friction may be neglected when compared with the journal-friction. Taking the value 1·6 lb. per ton to represent journal-friction for a journal 4·1 inches in diameter, and a wheel 41 inches in diameter, the formula for journal-friction may be written:—

$$16 \times \frac{\text{journal-diameter in inches}}{\text{wheel-diameter in inches}} \text{ lbs. per ton.} \quad . \quad (9)$$

Flange-Action.—The resistance due to flange-action will depend upon the type of coach used, whether a bogie- or a four-wheeled coach, also upon the length of the wheel-base and upon the play between the flanges and the rails.

If W_c W_b represent the weights in tons of the whole bogie-car and of two bogie-trucks, the resistance due to flange-action at V miles an hour for bogie-coaches will be

$$2 \cdot 94 \text{ V} \frac{c}{b} \frac{W_b}{W_c} \text{ lbs. per ton} \quad . \quad . \quad . \quad . \quad (10)$$

and for four-wheeled coaches

$$2 \cdot 94 \text{ V} \frac{c}{b} \text{ lbs. per ton} \quad . \quad . \quad . \quad . \quad (11)$$

where c is the total play between flanges and rails in inches, and b is the wheel-base in feet.

Air-Resistance.—The total air-resistance at 60 miles per hour in Mr. Barbier's tests was 5·7 lbs. per ton in both cases. If B and R signify the trains made up of bogie and four-wheeled coaches respectively, the average weight of the B trains was 197 tons and of the R trains 162 tons, making the air-resistance 1,123 and 923 lbs. for the two trains respectively.

In these tests the front pressure need not be considered, as in each case the first coach was entirely screened by the dynamometer-car. The rear suction, according to Table I, for a flat-ended vehicle, is equal to 0·50 lb. per square foot of exposed area above rails. The side and top friction is unknown, but will be the same function of the exposed area in each case.

Taking first the R trains, the over-all width was 9·1 feet, the height above rails 11·2 feet, the exposed cross area 102 square feet, and the rear suction 51 lbs., leaving 872 lbs. for the side and top effect. The periphery of the sides and top was 23·5 feet, the length of the train 470 feet, the exposed side and top area 11,060 square feet, making the side and top air-friction 79 lbs. per 1,000 square feet of exposed area.

The over-all width of the B trains was 10·0 feet, the height above rail 12·3 feet, the exposed cross area 123 square feet, and the rear suction 62 lbs., leaving 1,061 lbs. for the side, top, and bogie-friction. The side and top periphery was 26·6 feet, the length of the train 377 feet, the exposed area 10,028 square feet, and the air-effect, at 79 lbs. per 1,000 square feet, being thus 792 lbs., leaves 269 lbs. as the bogie-friction. The average number of bogie-trucks in the B trains was 11·6, making the air-friction per truck 23·2 lbs.

The components of air-resistance in Mr. Barbier's tests may thus be summarized as follows:—

TABLE III.

		Air-Resistance at 60 Miles per Hour.	
		Bogie-Coaches.	Four-wheel Coaches.
Side and top friction at 79 lbs. per 1,000 square feet	Lbs.	792	872
Rear suction at 0·50 lb. per square foot of cross area	„	62	51
Bogie-friction at 23·2 lbs. per truck	„	269	0
		1,123	923
Weight of train	Tons	197	162
Air-resistance	Lbs. per Ton	5·7	5·7

The four components of air-resistance may therefore be generally expressed as follows:—

TABLE IV.

Front pressure . . . Rear suction . . .	As given in Table I, depending on the profile.
Side and top friction .	79 lbs. per 1,000 square feet of side and top area.
Bogie-friction . . .	23 lbs. per bogie-truck.

From these values at 60 miles per hour the air-resistance at any other speed can be computed.

VERIFICATION OF RESISTANCE-FORMULAS.

The tests from which the formulas for train-resistance were derived in the preceding section may be accepted as conclusive for the type of trains tested and for a line similar to that on which they were made. It is evident, however, that before a claim can be established for the accuracy of their application to railways in general it must be shown that these formulas enable one to predetermine with precision the resistance of trains made up of rolling stock different from that tested on the Northern of France Railway, and running on other lines. For this purpose it will be necessary to choose a number of thoroughly reliable tests, made on different classes of rolling stock and on different railways, for which the required data are available, and compare the resistances thus obtained with those determined by means of the above formulas.

Reference has already been made to the difficulty of finding train-resistance tests complete with the necessary data whose results may be accepted as reliable. The following have, however, been selected, as representative tests fulfilling the required conditions.

(1) Tests of eight-wheeled bogie passenger-coaches made by Mr. J. A. F. Aspinall on the Lancashire and Yorkshire Railway.

(2) Tests of four-wheeled goods-wagons made on the London and North-Western Railway.

(3) Tests of eight-wheeled bogie goods-wagons made on the New York, Ontario and Western Railway.

(4) Tests of eight-wheeled electric bogie-coaches made by the St. Louis Electric Railway Test Commission.

(5) Tests of twelve-wheeled electric bogie-coaches made on the Zossen-Marienfelde Railway.

(1) *Eight-Wheeled Bogie Passenger-Coaches on the Lancashire and Yorkshire Railway.*—The tests made by Mr. Aspinall and described in the Paper read by him before The Institution,[1] are probably the most accurate train-resistance tests made in England. They were made on bogie-coaches differing materially from the bogie-coaches tested by Mr. Barbier, being lighter, with shorter bogie wheel-base, and less play between the flanges and the rails. The trains tested varied in the number of coaches of which they were made up; by far the larger number of tests were made with the five-coach train, over two hundred observations being plotted as the basis of the resistance-curve, thus securing to the results of these tests greater accuracy in comparison with the others; the five-coach train will therefore be selected and its resistance calculated by the formulas and compared with that obtained in the tests.

Journal-Friction.—The journals were 4 inches and the wheels 42 inches in diameter, making the journal-friction 1·5 lb. per ton.

Flange-Action.—The weight of the whole bogie-coach was 21·0 tons, of the bogie-trucks 4·52 tons each, the wheel-base was 6·5 feet, the play between flanges and rails $\frac{3}{4}$ inch. From the formula the flange-action is thus 8·8 lbs. per ton at 60 miles per hour.

Air-Resistance.—The coaches were 8·0 feet wide and their sides 8·5 feet high; the train was 285 feet long, and the area of sides and top thus 7,120 square feet, which at 79 lbs. per 1,000 square feet gives 562 lbs. pressure at 60 miles per hour. The leading coach had 32 square feet of flat surface exposed in front above the tender. Assuming that the mean pressure, owing to the shielding of the engine, is 4·1 lbs. per square foot, the resistance is 131 lbs. The

[1] "Train-Resistance," Minutes of Proceedings Inst. C.E., vol. cxlvii, p. 155.

cross area above the rails was 92 square feet, making the suction at the rear 46 lbs. The resistance due to the bogie-trucks at 23 lbs. each was 230 lbs. The total air-resistance is thus:—

TABLE V.

	Lbs.
Front pressure (32 square feet at 4·1 lbs.)	131
Rear suction (92 square feet at 0·50 lb.).	46
Side and top friction (7,120 square feet at 79 lbs. per 1,000)	562
Bogie-friction (10 trucks at 23 lbs.)	230
	969
Total weight of train in tons	115·21
Air-resistance in pounds per ton at 60 miles per hour .	8·4

In the following Table are given the resistances at other speeds deduced from those at 60 miles per hour, and the actual values obtained by test, as taken from Mr. Aspinall's curve. These results are also shown graphically in *Fig. 4*, the circles indicating the resistances observed in the tests.

TABLE VI.

Speed, miles per hour	10	20	30	40	50	60	70	80
Resistances, journal- .	1·5	1·5	1·5	1·5	1·5	1·5	1·5	1·5
,, flange- .	1·5	2·9	4·4	5·9	7·3	8·8	10·3	11·7
,, air- . . .	0·2	0·9	2·1	3·7	5·8	8·4	11·3	14·8
Total	3·2	5·3	8·0	11·1	14·6	18·7	23·1	28·0
By test	3·4	5·2	7·6	10·7	14·1	18·1	23·0	28·0
Error per cent. . . .	−6	+2	+5	+4	+4	+3	+1	0

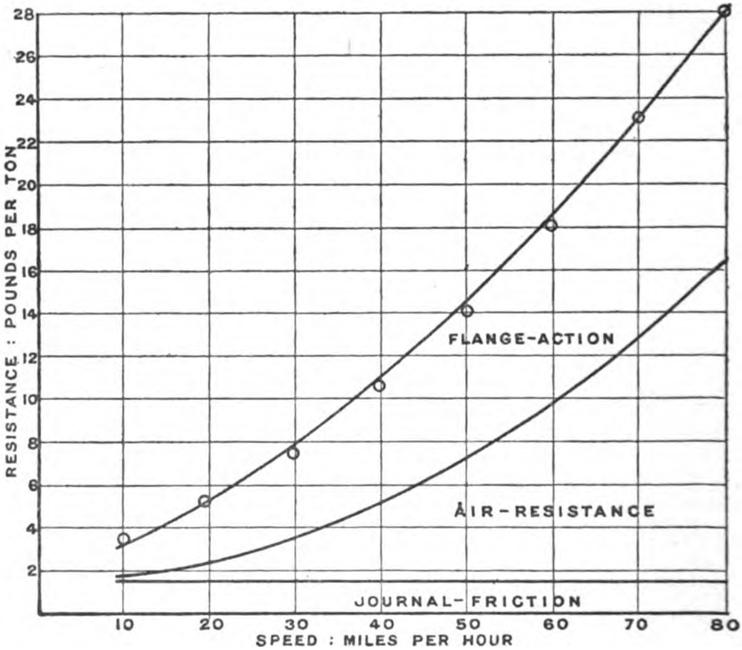
It will be seen that the greatest error is 6 per cent., whilst the mean error does not exceed 3 per cent.

(2) *Four-Wheeled Goods-Wagons on the London and North-Western Railway.*—In 1893 an exhaustive series of tests was carried out in this country and in America with the object of comparing the tractive resistance of English four-wheeled goods-wagons with American bogie goods-wagons. The tests in this country were carried out on the London and North-Western Railway, and those in America on the New York, Ontario and Western Railway. A full account of the tests, with reproduction of the dynamometer and speed records, and profile of the line, has been published in the *Railroad Gazette.*[1] The tractive resistance will be predetermined by the aid of the resistance-formulas and then compared with the results obtained in the tests.

[1] 1894, p. 225.

In each case a train of loaded wagons was made up and run over a considerable distance, the test-run on the London and North-Western Railway being 77 miles, from Rugby to Willesden. An automatic record was taken of the draw-bar pull on the dynamometer-car behind the engine. The mean tractive effort was obtained from the area of the dynamometer record after allowing for the difference of level between the start and the finish. The speed is taken to be the mean speed over the whole run exclusive of stops, and was 16·5 miles per hour in the English and 19·0 miles per hour in the American tests.

Fig. 4.



The test-train consisted of fifty-seven 10-ton coal-wagons and three brake-vans. The average tare of each wagon was 5·4 tons, and its load 7·44 tons; the tare behind the dynamometer-car was thus 326 tons, and the load 446 tons, making a total of 772 tons.

Journal-Friction.—The journals were 4¼ inches and the wheels 33 inches in diameter, making the journal-friction 2·1 lbs. per ton.

Air-Resistance.—The wagons were 8 feet wide, their sides 5 feet high, and the total length of the train 1,189 feet, giving an exposed

area of 21,400 square feet, and an air-resistance of 1,690 lbs. at 79 lbs. per 1,000 square feet. The three brake-vans projected each 3 feet above the wagons, offering 72 square feet of exposed front area, and an air-pressure of 294 lbs. at 4·1 lbs. per square foot. The cross area above rails was 64 feet, giving a rear suction of 32 lbs. The total air-resistance is thus 2,016 lbs. at 60 miles an hour, or 152 lbs. at 16·5 miles per hour, that is, 0·2 lb. per ton of total weight hauled.

Flange-Action.—The wheel-base was 9 feet. The play between flanges and rails was ½ inch when new, but no record was kept of the play as tested. Allowing ⅛ inch for wear, and taking the play as ⅝ inch, the flange-resistance becomes 3·4 lbs. per ton.

The total tractive resistance at 16·5 miles per hour is thus:

TABLE VII.

Journal-friction	2·1 lbs. per ton.
Flange-action	3·4 ,, ,,
Air-resistance	0·2 ,, ,,
Total	5·7 ,, ,,

The mean draw-bar pull as obtained from the dynamometer-record, after making allowance for difference of level, was 6·0 lbs. per ton of total load hauled, or 5 per cent. above the calculated resistance.

(3) *Eight-Wheeled Bogie Goods-Wagons on the New York, Ontario and Western Railway.*—The test-train was made up of twenty-four bogie-wagons with an average weight of 10·7 tons each and a load of 23·4 tons, the total tare being 256 tons, the load 560 tons, making altogether 816 tons behind the dynamometer-car.

Journal-Friction.—The journals were 4¼ inches and the wheels 33 inches in diameter, making the journal-friction 2·1 lbs. per ton.

Flange-Action.—The bogie-wagons weighed 34·1 tons loaded, and the bogie-trucks weighed 2·39 tons each; the play between flanges and rails was ½ inch, and the wheel-base 4 feet 10½ inches, hence the flange-resistance at 19 miles per hour is 0·8 lb. per ton.

Air-Resistance.—The wagons were 8 feet wide with sides 5 feet high, the length of the train was 751 feet, making the exposed area 13,500 square feet, and the resistance 1,070 lbs. at 60 miles per hour. The cross area was 64 square feet, making the rear suction 32 lbs. The bogie-friction for forty-eight bogies at 23 lbs. is 1,104 lbs. The total air-resistance at 60 miles per hour is thus 2,170 lbs., or 218 lbs. at 19 miles per hour, that is 0·3 lb. per ton.

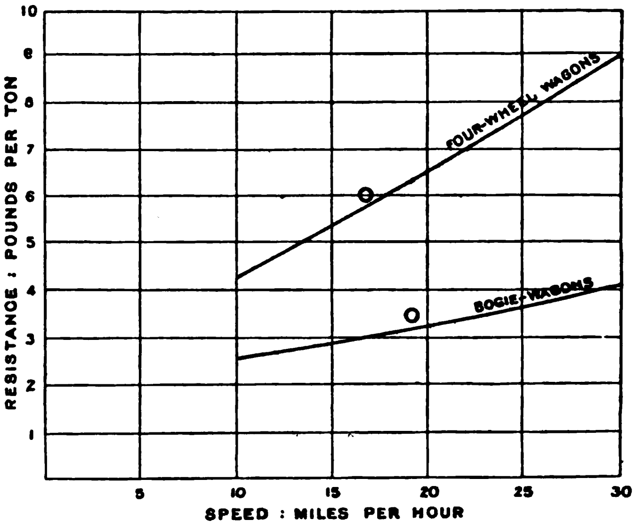
The total tractive resistance at 19 miles per hour as deduced from the formulas is thus:

TABLE VIII.

Journal-friction	2·1 lbs. per ton.
Flange-action	0·8 " "
Air-resistance	0·3 " "
Total	3·2 " "

The mean tractive effort obtained in the test was 3·5 lbs. per ton, the calculated value thus being 8½ per cent. less than the observed value. *Fig. 5* gives the resistance-curves of these two classes of

Fig. 5.



wagons at speeds ranging from 10 to 30 miles per hour; the circles indicate the results of the tests.

(4) *Eight-Wheeled Electric Motor-Coaches of the Indiana Union Traction Company.*—The train-resistance tests of the St. Louis Electric Railway Test Commission were conducted on about 5 miles of line belonging to the Indiana Union Traction Company, with 70-lb. rails set to standard gauge on oak sleepers and gravel ballast in first-class condition. The observations were taken in every case when the speed was uniform, the only corrections introduced being for gradients which were very slight.

The train tested consisted of one motor-coach, weighing altogether 33·2 tons, equipped with four 75-HP. motors mounted on two Baldwin M.C.B. inter-urban trucks with Gibb cradle-suspension. The coach-body was 51 feet 4 inches over all in length, with vestibules of standard profile at each end.

Journal-Friction.—The wheels were 37½ inches in diameter. The journals had a diameter of 4¼ inches. From the formula the journal-friction is 1·8 lb. per ton.

Track-Resistance.—Taking the inclination of the rail under the leading axle at 1 in 400, the track-resistance for the four axles is 0·7 lb. per ton.

Flange-Action.—The weight of each truck was 4·26 tons, and of two motors 4·02 tons, making the weight of one truck equipped with motors 8·28, and the total weight of the two trucks 16·56 tons. The wheel-base of the trucks was 6 feet, and the play between flanges and rail $\frac{7}{16}$ inch. From the formula the flange-action at 60 miles per hour is 6·4 lbs. per ton.

Air-Resistance.—The total length of the coach was 51·25 feet, the side and top periphery 28 feet, giving an area of 1,435 square feet, and an air-resistance of 113 lbs. at 60 miles per hour. The height from rail to top of roof was 13·5 feet, width 9·1 feet, cross area 123 square feet. The force of the air on the front and rear of a car with standard vestibule according to Table I is 5·93 lbs. per square foot at 60 miles per hour, hence the combined force on front and rear was 730 lbs. The resistance of the rear bogie was 23 lbs. The total air-resistance at 60 miles per hour is thus 865 lbs., or 26·1 lbs. per ton.

Side Wind.—The direction of the track on which the tests were made lay north-east and south-west, and during the whole period of the tests a north-west wind was blowing averaging 12 miles per hour. The exposed side area of the coach was 690 square feet, making the total force of the side wind, at 8·7 lbs. per square foot, equal to 240 lbs. With a coefficient of friction of 0·3 the resistance in the direction of motion is thus 2·2 lbs. per ton for all speeds.

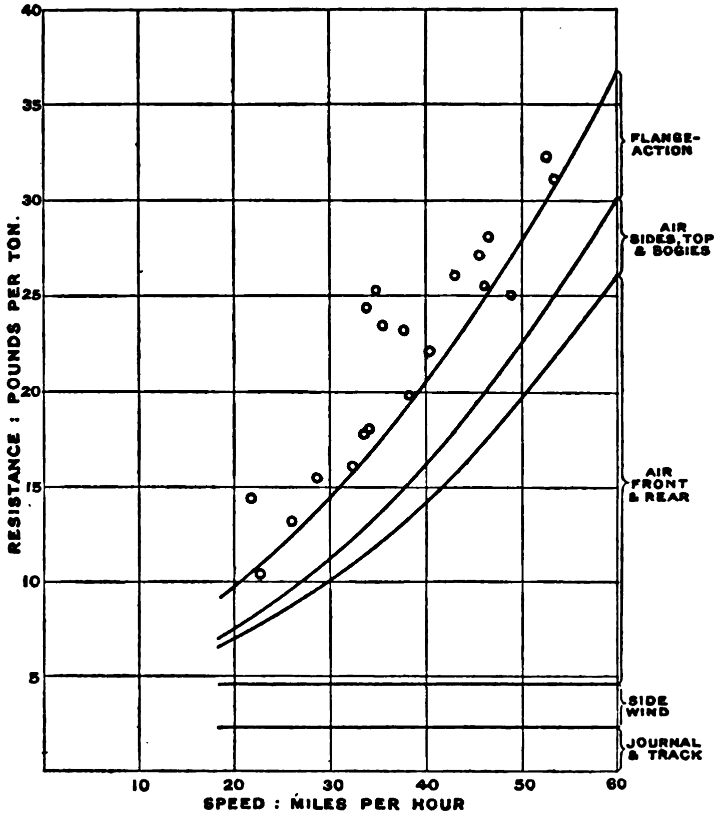
The total tractive resistance at 60 miles per hour is thus:

TABLE IX.

Journal-friction	1·8	lbs.	per ton.
Track-resistance	0·7	,,	,,
Flange-action	6·4	,,	,,
Air-resistance	26·1	,,	,,
Side wind	2·2	,,	,,
Total	37·2	,,	,,

The resistances at other speeds have been calculated from those at 60 miles per hour and plotted as a curve in *Fig. 6.* In the Report of the Test Commission the results of twenty separate observations are given: these have been reproduced[1] in *Fig. 6.* On comparing

Fig. 6.



these with the curve it will be seen that the agreement is very close. The calculated results are too low throughout by about 1 lb. per ton, the difference probably being due to under-estimated track-resistance.

(5) *Twelve-Wheeled Electric Bogie-Coaches on the Zossen–Marienfelde Railway.*—Amongst the most reliable tractive-resistance tests on

[1] Having found an error in the plotting of the curves published in the Commission's Report, the Author has thought it advisable to reproduce the actual observations, adjusting the figures to the ton of 2,240 lbs. instead of the ton of 2,000 lbs. used in the Report.

electrically-driven motor-coaches are those made by the "Studiengesellschaft fuer elektrische Schnellbahnen" on the experimental high-speed coaches at Zossen. An account of these tests may be found in the Paper[1] read by Mr. Alexander Siemens before the Institution of Electrical Engineers.

These tests were made on single coaches, each coach having two six-wheeled bogies with two motors per bogie. The tractive resistance was ascertained by running the coach up to a high speed and then allowing it to coast on its own momentum, observations being made to determine the retardation.

The tractive resistance will now be calculated by the formulas and compared with the actual tractive effort observed in the tests.

Journal-Friction.—The driving-wheels were 49·2 inches in diameter and the journals 6 inches, giving 1·95 lb. per ton. The weight of the car was 83 tons, making the journal-friction 162 lbs.

Track-Resistance.—Taking the inclination of the rail as 1 in 400, the track-resistance for six axles is 39 lbs.

Flange-Action.—The weight of each bogie complete with motors, wheels and axles was 21·4 tons, the wheel-base was 12·5 feet, and the play between flanges and rails 25 millimetres. Hence the flange-resistance at 60 miles per hour is 595 lbs.

Air-Resistance.—The car was 9·4 feet wide, 14·0 feet high above rails to top of roof, giving 132 square feet of exposed cross area. The ends of the car were shaped as in Profile 3, *Fig. 1*, offering a resistance to the air midway between Profiles 2 and 4. The force on front and rear will be taken as the mean between these, that is, at 4·33 lbs. per square foot, or a total of 572 lbs. Owing to the great length of the bogies, and the depth of the underframe between the bogies, the side resistance will be taken as from the rail-level, and this will include the bogie-resistance. The exposed periphery is 37·4 feet, the length 72 feet, giving an exposed area of 2,690 square feet, and a resistance of 212 lbs. The total air-resistance is thus 784 lbs.

The total tractive resistance at 60 miles per hour as obtained from the formulas is thus:

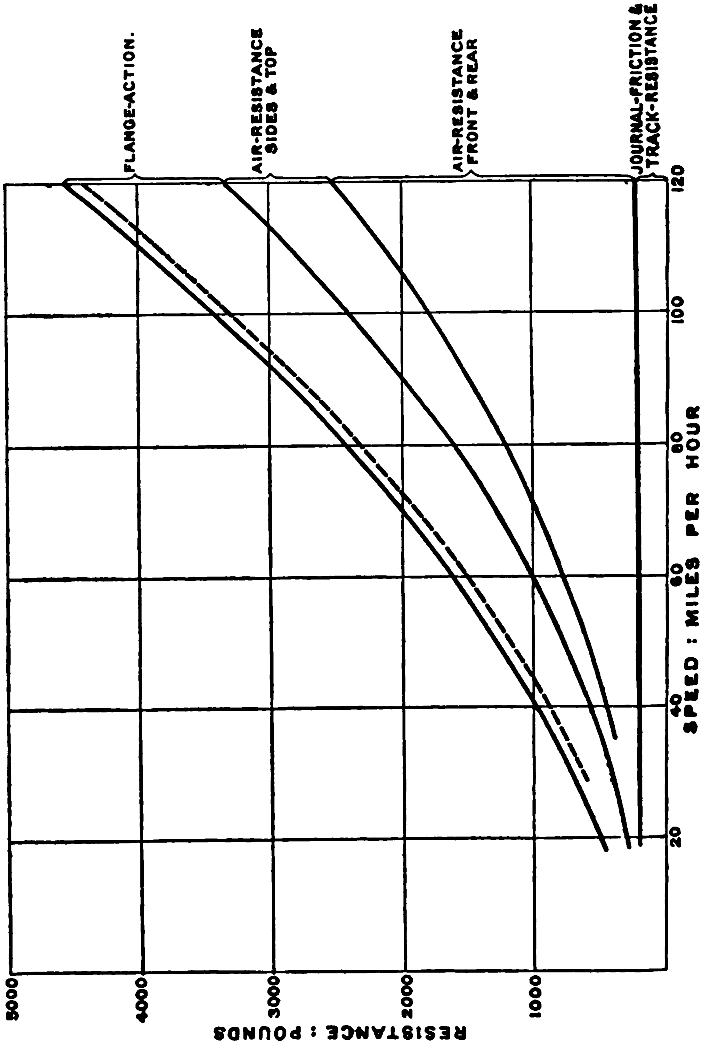
TABLE X.

	Lbs.	Lbs. per Ton.
Journal-friction	162	1·95
Track-resistance	39	0·47
Flange-action	595	7·17
Air-resistance	784	9·45
Total	1,580	19·04

[1] "High-Speed Electric Railway Experiments on the Marienfelde-Zossen Line." Journal of the Institution of Electrical Engineers, vol. 33 (1904), p. 894.

The tractive resistance found by the retardation method at this speed was 1,460 lbs., or 7½ per cent. less than the value obtained from the formulas. The resistances at other speeds, up to 120 miles

Fig. 7.

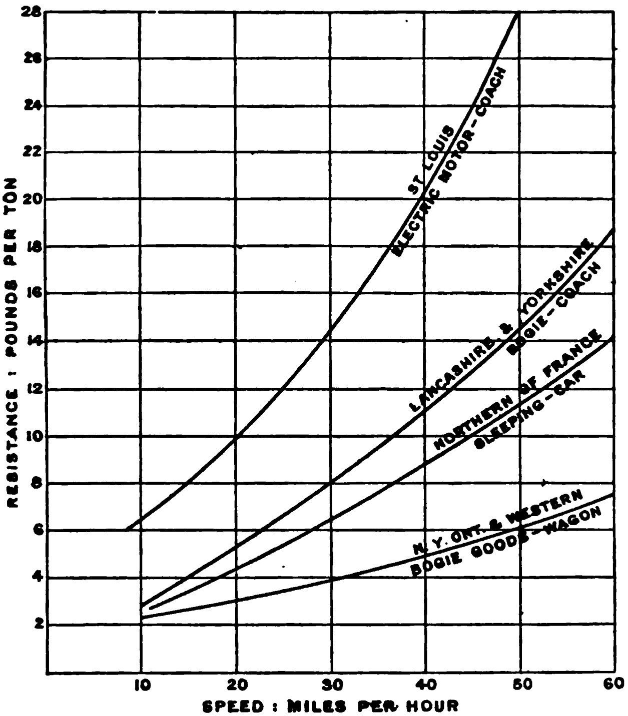


per hour, have been calculated and plotted as a curve in *Fig. 7.* Curves ·g the components of the total resistance are also shown.

The results obtained by the Syndicate in their tests at Zossen on the tractive resistance are given in their report in the form of a curve, which has been reproduced as a dotted line in *Fig. 7*. The retardation tests were carried out at speeds up to 75 miles per hour, the resistances at higher speeds being deduced from the observations at the lower speeds.

The resistances obtained from the formulas are throughout greater than those given by the test-curve by a nearly constant amount

Fig. 8.



of 100 lbs., indicating that the difference is probably due to an over-estimation of the journal-friction. The Syndicate have not published any details of the methods adopted for lubricating the journals of the high-speed coaches, nor the results of their investigations into the coefficient of friction, so that at present it can only be said that the difference appears to be due to the method of lubrication employed.

It will be seen that in each of the five cases dealt with above the

train-resistance can be predetermined with considerable accuracy. The cases selected represent conditions varying from those of a four-wheeled goods-wagon running at 16 miles per hour to those of a high-speed twelve-wheeled electric bogie-coach. The extent to which these varying conditions influence train-resistance is well shown by *Fig. 8*, in which are given side by side on the same scale the resistance-curves of the Northern of France bogie-coaches, of the Lancashire and Yorkshire bogie-coaches, of New York, Ontario and Western bogie goods-wagons, and of the electrical motor-coaches tested by the St. Louis Commission.

Practical Conclusions based upon the Resistance-Equations.

Journal-Friction in its Relation to Train-Resistance and its possible reduction by the use of Roller-Bearings.—Journal-friction constitutes a large portion of the total resistance of most classes of rolling stock, and its reduction is a matter of practical importance. There does not seem to be any prospect of effecting a reduction by the use of so-called anti-friction bearing-metals, for whatever merits these may possess in the way of durability and cool running, reliable tests show that under normal conditions of journal speed and pressure on railways the coefficient of friction is not materially different from that obtained under similar conditions with the metals in ordinary use. The only practical method of making any considerable reduction in journal-friction appears to be the use of roller-bearings. Attention has been directed hitherto mainly towards the use of roller-bearings as a means of overcoming starting-resistance. While it is quite true that these bearings under test show a great reduction of starting-resistance, this is not the most important function that they may be expected to fulfil. The value of roller-bearings in railway-traction lies in the reduction of running-resistance and the consequent saving of energy.

In 1900 Professor John Goodman made some experiments at the Yorkshire College, Leeds, to ascertain the coefficient of friction of roller-bearings running under ordinary railway conditions. In these tests the bearing consisted of fourteen rollers ¾ inch in diameter and 5·9 inches long, the journal-diameter being 3¾ inches, and the area of the bearing-surface 22·1 square inches. The bearing was run perfectly dry with varying loads, and at speeds corresponding to 15 and 45 miles per hour on a 42-inch wheel. The coefficient of friction, measured by an apparatus specially designed by Professor Goodman, was found to decrease inversely with the load and to decrease with

an increase of speed; the results obtained are given in the following Table.

TABLE XI.

Load in Lbs. per Square Inch.	Coefficient of Friction with Roller-Bearings.	
	At 15 Miles per Hour.	At 45 Miles per Hour.
45	0·0090	0·0054
90	0·0052	0·0037
136	0·0040	0·0028
181	0·0036	0·0024
226	0·0033	0·0022
272	0·0031	0·0022
316	0·0030	0·0022

Taking 250 lbs. per square inch as a normal pressure, it will be seen that the coefficient of friction at 15 miles per hour with these bearings is 0·0032, while at 45 miles per hour it is 0·0022. With ordinary bearings and pad lubrication, the mean of Mr. Stroudley's results gave 0·0077 as the coefficient for 280 lbs. per square inch, showing that the coefficient of friction with roller-bearings is 42 per cent. of what it is with ordinary bearings at 15 miles per hour, and 29 per cent. at 45 miles per hour. The friction of roller-bearings is thus about one-third of that of ordinary bearings.

The extent of the saving to be effected by the use of these bearings will depend upon the class of rolling stock under consideration and upon the speed at which it is to be run. For example, it will be seen from the results of the tests on the Lancashire and Yorkshire bogie-coaches that journal-friction varies from 28 per cent. of the total resistance at 20 miles per hour to 8 per cent. at 60 miles an hour. The possible saving due to roller-bearings is therefore 18 per cent. of the total at the former speed and only 5 per cent. at the latter. It is clear that the speed greatly influences the value of these bearings as savers of energy. Taking 40 miles per hour as the mean speed at which these coaches would be run, the saving would be 9 per cent. The saving here is small on account of the large flange-resistance. With coaches of the London and South-Western type run at 40 miles per hour on that line with a flange-play of $\frac{3}{8}$ inch, journal-friction is 26 per cent. of the total, and the saving due to roller-bearings at 40 miles per hour would be 17 per cent.

Some trials were recently made on the Eastern Bengal State Railway in India to test the practical value of roller-bearings. Two trains of identical composition, one fitted with roller-bearings, the

other with ordinary brass bearings, were run against each other on the same service between Calcutta and Rajbari, a distance of 145·5 miles. The same engines and vehicles were used throughout. Each train consisted of six bogie-coaches weighing 166·5 tons, with a mail-engine and tender weighing 85·5 tons, in all 252 tons. The mean running-speed was 34·4 miles per hour.

The bogie-coaches weighed 27·5 tons each, the flange-play was $\frac{5}{8}$ inch, and the flange-resistance, calculated from the formula, was 2·7 lbs. per ton, the air-resistance being 1·8 lbs. per ton. The diameters of the wheels and journals were 43 inches and 4 inches respectively, making the journal-friction 1·5 lb. per ton with ordinary brasses, and 0·5 lb. per ton with roller-bearings. The total resistances in the two cases are thus 6 and 5 lbs. per ton, showing a saving on the coaches of $16\frac{1}{2}$ per cent. due to roller-bearings. The weight of the coaches was two-thirds of that of the whole train. Data are not available for estimating precisely the resistance of the locomotive, but assuming that it was as much as that of the coaches, the saving for the whole train would be two-thirds of 16·5, or 11 per cent.

In the trials, three trips were made each way with both trains, the distance covered by each train being 873 miles, and a careful record was kept of the coal- and water-consumption. The results obtained are given in the following Table:—

TABLE XII.

	Brass Bearings.	Roller-Bearings.
Train-mileage	873	873
Coal consumed	28,342 lbs.	24,840 lbs.
Water ,,	217,240 ,,	191,630 ,,
Coal-ratio	100 per cent.	87·6 per cent.
Water- ,,	100 ,,	88·2 ,,

The mean saving due to roller-bearings is thus 12 per cent. as compared with 11 per cent. obtained from the formula.

The Bengal-Nagpur Railway Company is having a number of goods-wagons equipped with roller-bearings with a view to test their efficiency. The wagons on which these bearings are being fitted have a wheel-base of 11 feet 6 inches; the flange-play is $\frac{5}{8}$ inch, and the flange-resistance at 25 miles per hour is 4 lbs. per ton. The air-resistance at this speed is 0·3 lb. per ton. The diameter of the journals is 5 inches, and of the wheels 43 inches, making the journal-friction 1·9 lb. with gun-metal bearings. The total resistance is thus 6·2 lbs. per ton. Recent trials with a dynamometer-car show that the resistance of these wagons when loaded with $22\frac{1}{4}$ tons of coal, and running at 25 miles per hour, is 6·7 lbs. per ton, or 8 per

cent. greater than the value obtained from the formulas. By the use of roller-bearings the journal-friction should be reduced to 0·6, making a saving on the total, at this speed, of 21 per cent.

With four-wheeled wagons flange-action is the most important item of train-resistance. Thus for the Bengal-Nagpur wagon flange-action is 64 per cent, and the journal-friction, with ordinary brasses, 31 per cent. of the total resistance. But with bogie-wagons the proportion is nearly reversed. Thus, with bogie-wagons of the type of the Leeds Forge 30-ton wagons, running at 25 miles per hour on a line with the same flange-play as that on the Bengal-Nagpur Railway, flange-action is 33 per cent. and journal-friction 58 per cent. of the total. Hence, while the saving due to roller-bearings with the four-wheel wagons is two-thirds of 31 per cent., or 21 per cent., the possible saving with the bogie-wagons is two-thirds of 58 per cent., or 39 per cent. It follows from this that the most promising field for the introduction of roller-bearings, or indeed of any device tending to reduce journal-friction, is to be found in bogie goods-wagons.

In estimating the saving it has been assumed that the speeds quoted are continuously maintained. If there are frequent stops the energy expended in acceleration has to be added to that required to overcome train-resistance, with the result that the saving due to roller-bearings, expressed as a percentage of the total, is much reduced. It is true that roller-bearings diminish the friction of starting to a greater extent than they diminish the friction of running, but this is only during the first few revolutions of the journal, and the period is so small when compared with the whole time of acceleration that the saving on this account is negligible. For this reason the percentage saving of energy due to the use of roller-bearings diminishes in proportion as the stops are more frequent, and for the same reason the saving observed in actual working will always be slightly less than that based on the assumption of continuous running without stops.

Influence of the Truck on the Resistance of Bogie-Coaches.—The use of bogies has an important influence on flange-action, and may result in a large reduction of the total train-resistance. The saving, however, will depend upon the weight of the bogie and its wheel-base, and it may happen, if the wheel-base is short and the bogie heavy compared with the weight it has to carry, that a bogie-coach will be as hard to haul as one with a rigid wheel-base. The bogie coach must necessarily have a comparatively short wheel-base, and this in itself is a disadvantage; but if the reduction in the wheel-base is accompanied by more than a corresponding reduction in the weight of the mass subject to side oscillation, there will on the whole be a

gain. Thus, in the case of the coaches tested on the Northern Railway of France, the bogie-coaches had a wheel-base of 8·2 feet against 17·7 feet for the four-wheeled coaches, the one being 46 per cent. of the other; but the weight of the two bogies was only 30 per cent. of that of the whole coach, showing a net reduction of flange-action in the proportion of 46 to 30. Whereas, with the type of bogie-coach tested on the Lancashire and Yorkshire Railway, the wheel-base was 37 per cent. of a 17·7-foot base, while the weight of the bogies was 43 per cent. of the total, the reduction in the wheel-base thus counterbalancing the saving due to weight, and making the flange-action of the bogie-coaches the greater of the two.

The accompanying Table gives the wheel-base and weights of the coaches referred to above, together with those of some other passenger-coaches. The Table also gives the flange-resistance, calculated for a speed of 40 miles per hour and a flange-play of $\frac{3}{4}$ inch,

TABLE XIII.

Railway.	Wheels per Coach.	Total Weight	Weight of Two Bogies Complete.	Wheel-base.	Flange-Resistance at 40 Miles per Hour, $\frac{3}{4}$-inch Play.	Total Resistance.	Type of Coach.
	No.	Tons.	Tons.	Feet.	Lbs. per Ton	Lbs. per Ton	
Northern of France . .	4	11·6	..	17·7	5·0	9·1	Passenger-coach.
Bavarian State .	6	21·1	..	30·4	2·9	7·0	Ditto.
Ditto	8	38·3	11·2	8·2	3·1	7·2	Ditto.
Lancashire & Yorkshire . .	8	21·0	9·0	6·5	5·9	10·0	Ditto.
Great Central .	8	30·0	11·0	8·0	4·0	8·1	Ditto.
London & South-Western . .	8	32·0	9·5	8·0	3·3	7·4	Dining-car.
International Sleeping-Car Company . .	8	33·8	10·0	8·2	3·2	7·3	Sleeping-car.
Ditto	8	37·5	11·8	8·2	3·4	7·5	Ditto. New pattern.
Ditto	8	32·4	11·8	8·2	3·9	8·0	Dining-car.
Great Western .	8	33·0	11·8	9·0	3·1	7·2	Composite coach.
Great Central .	12	34·0	13·4	12·0	2·9	7·0	Passenger-coach.
Great Western .	12	38·8	13·5	11·5	2·7	6·8	Dining-car.
International Sleeping-Car Company . .	12	47·6	19·1	12·1	2·9	7·0	Ditto.

and the total resistance assuming air- and rolling-resistance at 4·1 lbs. per ton as in Mr. Barbier's tests.

Effect of Electrical Driving on the Resistance of Bogie-Coaches. —In the case of electric traction, where the carriages are driven by motors geared to the axles, there is a large increase of the weight of the bogies, and a consequent increase in the train-resistance. Generally speaking, the weight of a motor-driven truck will be two to three times the weight of a trailing truck of equal wheel-base, the ratio of weight depending upon the output of the motors with which the motor-truck is equipped. The accompanying Table gives

TABLE XIV.

Railway.	Type of Truck.	Weight of one Truck Complete.	Weight of Half the Coach.	Wheel-Base.	Flange-Resistance.	Total Resistance.	Flange-Resistance per Ton on Centre Pin.	Power of Motors per Truck.
		Tons.	Tons.	Feet.	Lbs. per Ton.	Lbs. per Ton.	Lbs.	HP.
London & South-Western . . .	Trailing	4·75	16·0	8·0	3·3	7·4	4·7	..
Great Northern & City	Trailing	3·45	9·5	6·1	5·3	9·4	8·2	..
Ditto	Motor	6·00	14·1	6·1	6·2	10·3	10·7	125
Manhattan Electric	Trailing	3·30	9·0	5·6	5·9	10·0	9·2	..
Ditto	Motor	10·10	20·9	6·8	6·4	10·5	12·4	400
Indiana Union Traction . . .	Motor	8·28	16·6	6·0	7·3	11·4	14·5	150

the weights of motor and trailing trucks, with their wheel-bases, for three electrical railways, and for comparison, similar data for the eight-wheeled bogie-coaches of the London and South-Western Railway already described. The results are given for a speed of 40 miles per hour and a flange-play of $\frac{3}{4}$ inch, 4·1 lbs. per ton being added for air- and rolling-resistance. The flange-resistances and the total resistances are calculated in each case per ton of weight on the bogie-wheels.

An inspection of the Table shows that the resistance of the motor-trucks is considerably higher than that of the trailing trucks, the total resistance of the Indiana motor-trucks, for example, is 54 per cent. higher than that for the London and South-Western trailing trucks.

Reduction of the Resistance of Goods-Wagons by the Use of Bogies.—The influence of bogies on train-resistance is especially noticeable in the case of goods-wagons (see Table XV). The 10-ton

four-wheeled wagon with a rigid wheel-base of 9 feet, such as that used in the tests on the London and North-Western Railway already described, has a flange-resistance of 7·3 lbs. per ton at 30 miles an hour with ¾-inch flange-play. Adding journal-resistance 1·8 lb. per ton for ordinary bearings, and air-resistance 0·6 lb., the total resistance is 9·7 lbs. The ratio of tare to load is 0·54, and the power required to haul 100 tons of paying load at 30 miles per hour is 121 HP. If the wheel-base is lengthened to 12 feet, as with the 20-ton all-steel coal-wagon made by the Leeds Forge Company, which has a tare-ratio of 0·40, the flange-action at 30 miles per hour

TABLE XV.

	Load.	Tare.	Total Load.	Weight of two Trucks.	Tare-Ratio.	Wheel-Base.	Flange-Resistance per Gross Ton at 30 M. per Hr.	Total Resistance per Ton.	HP. per 100 Tons of Load	Flange-Resistance per Ton of Load.
	Tons.	Tons.	Tons.	Tons.		Ft. Ins.	Lbs.	Lbs.	HP.	Lbs.
					Rigid Wheel-Base.					
London & North-Western Railway	10·0	5·4	15·4	..	0·54	9 0	7·3	9·7	121	11·2
Leeds Forge .	20·0	8·0	28·0	..	0·40	12 0	5·5	7·9	89	7·7
					Bogie-Trucks.					
New York, Ontario & Western Railway	23·4	10·7	34·1	4·8	0·46	4 10½	1·9	4·3	51	2·8
Leeds Forge .	30·0	12·8	42·8	5·9	0·43	5 6	1·6	4·0	46	2·3
Carmaux . .	49·0	15·2	64·2	6·0	0·31	5 5	1·2	3·6	38	1·6

is 5·5 lbs., the total resistance 7·9 lbs., and the power per 100 tons of load 89 HP., showing a saving of 26 per cent. on the 10-ton wagon. Of this saving, 8 per cent. only is due to the improved tare-ratio, the remaining 18 per cent. being secured by lengthening the wheel-base from 9 to 12 feet. As far as the Author has been able to ascertain, this wagon represents the utmost that has been accomplished with a rigid wheel-base in the direction of economical goods-haulage.

The bogie goods-wagons tested on the New York, Ontario and Western Railway have a flange-resistance of 1·9 lb. per ton at 30 miles per hour with ¾-inch play; adding 2·4 lbs. for air- and journal-resistance as with the four-wheeled wagons, the total resistance becomes 4·3 lbs. per ton. The tare-ratio, with the load as tested, is 0·46, making the power required to haul 100 tons of

paying load 51 HP. This shows a saving of 42 per cent. over the lowest figure attainable with a four-wheeled wagon.

The 30-ton all-steel bogie-wagons of the Leeds Forge Company have trucks weighing 2·95 tons each, with a wheel-base of 5 feet 6 inches and tare-ratio of 0·43. From the formula these wagons have a flange-resistance of 1·6 lb., and a total resistance of 4·0 lbs. per ton, and require 46 HP. to haul 100 tons of load at 30 miles per hour, showing a still further reduction. The limit appears to be reached by the 50-tonne Fox-Arbel steel wagons made by the Forges de Douai for the Carmaux coal-mines; these have trucks weighing 3·0 tons each, with 5-foot 5-inch wheel-base and a tare-ratio of 0·31. By the formula the flange-resistance is 1·2 lb., the total resistance 3·6 lbs., and the power required for 100 tons load is 38 HP.

Relation between the Tractive Efforts required to Haul Loaded and Empty Bogie Goods-Wagons.—The flange-action per ton of bogie-wagons has been shown to depend upon the ratio of the weight of the two bogie-trucks to that of the whole wagon. It would follow from this that the greater the load carried by a bogie-wagon the smaller will be the resistance per ton of the total weight hauled.

It is a matter of general experience, though never satisfactorily explained, that the tractive effort per ton required to move a loaded bogie-wagon is less than that for the same wagon empty. A full description of one of the many tests that have been made to establish this fact may be found in the *Railroad Gazette* for the 14th April, 1899 (p. 262). In this test a train was made up of sixty-five bogie-wagons, the total length being 1,863 feet. The tare of each wagon was 10·6 tons, making the total weight empty behind the dynamometer-car 689 tons. The wagons were loaded with an average load of 25·3 tons each, making the total weight loaded 2,340 tons. The train was run loaded over a distance of 115 miles in one direction at 15 miles per hour, and back over the same line empty at about the same speed. Eight trips were made in each direction, and the means of the dynamometer-readings were compared. It was found that the average tractive effort per ton for the loaded wagons was 56 per cent. of that for the same wagons empty.

In the test the load on the brasses with the wagons full was 3·4 times as great as when the wagons were empty, and it has been suggested that the difference in the resistance may be due to the variation in the coefficient of friction of the journals with the increased load. But it has already been pointed out that under the conditions of lubrication obtaining in railway-carriage axles

there is very little variation in the coefficient of friction with increase of load, and the experiments made by Mr. Tower and others show that this variation is negligible, so that the reduction in the tractive effort per ton cannot be due to this cause. If, however, the fact that flange-action per ton depends upon the ratio of the weight of the bogie-trucks to that of the whole wagon be taken into account, the difference can be explained.

The necessary data for determining the resistance of the loaded and empty wagons used in the tests are not recorded, but the wagons were very similar to those used in the tests on the New York, Ontario and Western Railway, and the "loaded" and "empty" resistances for these wagons can be determined from the formulas.

Taking the weights as given above, namely, the weight of the wagon 10·6 tons empty and 35·9 tons loaded, the weight of the two bogie-trucks 4·78 tons, the wheel-base 4 feet 10½ inches, and the flange-play ½ inch, the flange-action at 15 miles per hour can be shown to be 0·6 lb. per ton loaded and 2·0 lbs. per ton empty. The journal-friction will be 2·1 lbs. per ton, and the air-resistance 0·2 lb. loaded and 0·6 lb. empty. The total resistances at 15 miles per hour are thus:

TABLE XVI.

	Lbs. per Ton. Loaded.	Lbs. per Ton. Empty.
Journal-friction	2·1	2·1
Flange-action	0·6	2·0
Air-resistance	0·2	0·6
Total	2·9	4·7

The total resistance loaded is thus 62 per cent. of that empty.

Incidence of Train-Resistance on Flange- and Rail-Wear.—The energy expended in overcoming flange-resistance is represented by wear of tires and rails. In order to effect a proper comparison of the amount of this action caused by different types of rolling stock, the flange-resistance should be estimated in pounds per ton of useful load carried. In the case of goods-wagons the flange-resistance can, for this purpose, be estimated per ton of capacity when fully loaded. The last column in Table XV gives these values for the different types of goods-wagons there referred to. Comparing, for example, the Leeds Forge four-wheeled and bogie wagons it will be seen that the flange wear per ton of capacity in the latter wagon is 30 per cent. of that of the former.

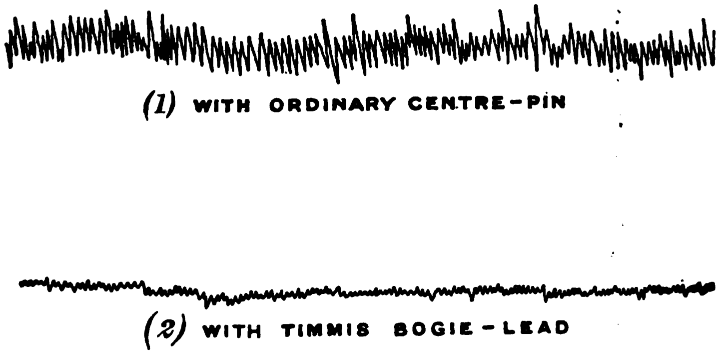
With passenger-coaches it is not convenient to estimate the flange-wear in pounds per ton of useful load, and in Table XIV it

has been taken per ton of load carried on the centre pin of the bogie. Taking the London and South-Western bogie as a standard of comparison, it will be seen that the wearing action of electrically-driven bogies is two to three times as great as with steam-drawn bogies.

Reduction of Flange-Action by Mechanical Contrivances.—Various suggestions have been made from time to time as to methods by which the oscillation of the bogie might be reduced, but as far as the Author is aware the Timmis bogie-lead is the only contrivance at present in actual operation which has this end in view. It may not be out of place to consider the saving that may be expected to result from its use.

In the Timmis bogie-lead, as is well known, the bogie is pushed from a point ahead of the centre pin, generally nearly over the

Fig. 9.



leading axle. Numerous tests have shown that the effect of this arrangement is to check the oscillations of the bogie and to convert the coach for the time being into one running on a practically rigid wheel-base whose length is equal to the distance between the leading axles of the two bogies, that is, to the distance between the centres of the bogies. Hence, the flange-resistance is no longer that of a bogie-coach but of one with a rigid wheel-base, and the saving, if any, will depend upon the length of that base, and upon its relation to the bogie wheel-base.

Fig. 9 shows the comparative movements in a horizontal plane of a point over the leading axle as observed with an oscillograph when running at 42 miles an hour; (1) with ordinary centre-pin arrangement, and (2) with the Timmis bogie-lead.

Taking the London and South-Western bogie-coach already

described, the flange-resistance at 40 miles per hour and $\frac{3}{4}$-inch play has been shown to be 3·3 lbs. per ton. If now the bogies are prevented from oscillating, and are driven from points over the leading axles, the coach will tend to run as with a rigid wheel-base equal to the distance between the bogie centres, which in this case is 39 feet. The flange-resistance with the same speed and play as before will then be 2·3 lbs. per ton. In the one case the bogies weighing 9·5 tons oscillate on an 8-foot wheel-base, in the other case the whole coach weighing 32 tons oscillates on a 39-foot wheel-base; the result is a reduction of oscillation and of flange-action in the proportion of 3·3 to 2·3. If the rolling- and air-resistance together amount to 4·1 lbs. per ton, the totals in the two cases are 7·4 and 6·4, showing a saving of 13 per cent.

The greater the weight of the bogies in proportion to their wheel-base the greater will be the saving due to this arrangement; in other words, the saving will be greatest where flange-action is most pronounced.

Effect of Side Play on Train-Resistance and its Possible Limitation.—The amount of play on the straight between the flanges and the rails is an important factor in determining the extent of flange-action. It will have been evident from the data already furnished in connection with the different tests above described that there is no uniformity in current practice as regards the amount of flange-play permitted. In illustration of this point, the flange-play on a number of railways is given in Table XVII. In England the flange-play varies from $\frac{3}{8}$ inch to $\frac{3}{4}$ inch. In the United States there is the same large range of variation, while some railways, not given in the Table, have as little as $\frac{1}{4}$ inch, to permit of trains running over city rails with grooves. On the Continent the play is considerably more.

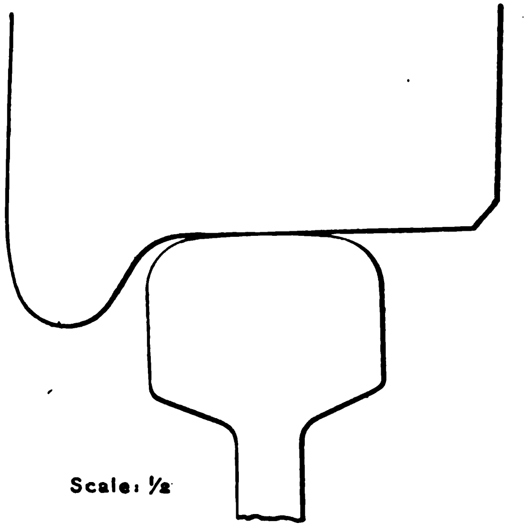
TABLE XVII.—FLANGE-PLAY.

	Inch.	Inch.
London and South-Western Railway	$\frac{3}{8}$	0·375
Great Western Railway	$\frac{1}{2}$	0·500
Great Central Railway	$\frac{5}{8}$	0·625
Lancashire and Yorkshire Railway	$\frac{3}{4}$	0·750
Indianapolis and Cincinnati Traction Company	$\frac{3}{8}$	0·375
Indiana Union Traction Company	$\frac{7}{16}$	0·437
New York, Ontario and Western Railway	$\frac{1}{2}$	0·500
Grand Rapids Railway	$\frac{5}{8}$	0·625
Manhattan Elevated Railway	$\frac{3}{4}$	0·750
Bengal-Nagpur Railway	$\frac{5}{8}$	0·625
	Millimetres.	Inch.
Eastern Railway of France	24	0·945
Zossen-Marienfelde Railway	25	0·985
Northern Railway of France	28	1·100

The amount of flange-play depends largely upon the shape of the rail- and flange-section. *Figs. 10* and *11* give two typical sections, *Fig. 10* being that adopted on the London and South-Western Railway with a total play of $\frac{3}{8}$ inch, and *Fig. 11* that used on the Bengal-Nagpur Railway with a total play of $\frac{5}{8}$ inch.

The results of the tests made on the Northern Railway of France show that at 40 miles per hour the flange-resistance with a play of 28 millimetres was 4·7 lbs. per ton, and the total resistance 8·8 lbs. If the same train were run with a play of $\frac{3}{4}$ inch, as on the Lancashire and Yorkshire Railway, the flange-resistance would be 3·2 lbs. and the total 7·3 lbs. per ton, showing a saving of 17

Fig. 10.



per cent.; with a play of $\frac{3}{8}$ inch, as on the London and South-Western Railway, the total resistance would be 5·7 lbs. per ton, making a saving of 35 per cent.

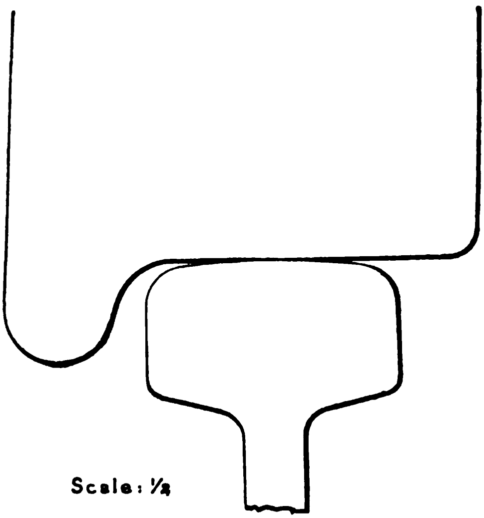
In the same way, if a goods-wagon, such for example as the Leeds Forge 20-ton four-wheeled wagon, is run at 30 miles per hour with a $\frac{3}{8}$-inch play, the total resistance is 34 per cent. less than when run at the same speed with a $\frac{3}{4}$-inch play. With a 30-ton bogie goods-wagon the saving under similar conditions would not be more than 20 per cent., owing to the fact that the flange-resistance is a smaller percentage of the total.

In view of the saving in train-resistance caused by small flange-

play, the question whether it may not be possible generally to reduce the play to a standard $\frac{3}{8}$ inch, as is at present done on the London and South-Western and other railways, appears to be worthy of serious consideration.

Relative Importance of Air-Resistance.—The relative importance of air-resistance as an item in the total tractive effort required to haul a train depends largely upon the speed. For goods-trains running at 16 to 20 miles per hour air-resistance is 4 to 10 per cent. of the total. For passenger-coaches running at 40 miles per hour the proportion is about 30 per cent., and at 60 miles per hour it is about 40 per cent. Thus, for example, the air-resistance

Fig. 11.



of the trains of bogie-coaches on the Northern Railway of France is 40 per cent., and on the Lancashire and Yorkshire it is 45 per cent. of the total tractive effort at 60 miles per hour. These figures do not include the resistance encountered by the front of the train; if this is taken equal to that of a standard vestibule and added to the other resistances, the whole air-resistance in the case of the Northern of France coaches is 48 per cent. of the total tractive effort. The resistance of the air with a train of bogie-coaches running at 60 miles per hour is thus about one-half of the total tractive effort.

The most important element of this resistance is the friction

of the air on the sides and top of the train, which is 24 per cent. of the total tractive effort. This friction amounts to 79 lbs. per 1,000 square feet of exposed area, and is two and a half times as great as it would be if the sides and top of the train were perfectly smooth.

The resistance due to the air-pressure in front and the suction behind accounts for 16 per cent. of the total tractive effort for a train of bogie-coaches. The experiments made by the St. Louis Electric Railway Test Commission show that it is possible to reduce this resistance by suitably shaping the ends of the front and rear coaches. The results indicate that the reduction effected by fitting the front with a parabolic profile would mean a saving of 6 per cent. of the total tractive effort required for a train of bogie-coaches at 60 miles per hour.

With trains consisting of one or two motor-coaches the portion of the total tractive effort expended in overcoming air-resistance may be greater than that with a train of several coaches. Thus, for the motor-coach used in the St. Louis tests, the air-resistance is 70 per cent. of the total. In such cases the friction of the air on the sides and top is small, being only 9 per cent., while the front and rear resistances amount to as much as 59 per cent. of the total tractive effort. The possible saving due to shaping the ends is thus 32 per cent.

The Author desires to express his thanks to Mr. Barbier of the Northern Railway of France, to the Engineers of the Great Western, the London and North-Western, the Lancashire and Yorkshire, the Great Central, the Bengal-Nagpur, and the New York, Ontario and Western railways; also to Mr. S. Warner, of the Carriage and Wagon Department, London and South-Western Railway, Professor Goodman, and to the Baldwin Locomotive Company for so fully replying to his inquiries and assisting him with data and information.

The Paper is accompanied by drawings and sun-prints, from which Plate 4 and the Figures in the text have been prepared, and by the following Appendix.

[APPENDIX.

APPENDIX.

Note 1.—Track-Resistance.

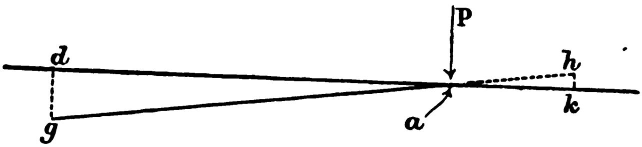
Let w be the weight in tons on the leading axle, h the depression under the axle in feet, $\frac{h}{k}$ the inclination of the rail produced by the depression. The load w is supported over a length $2\,k$ of the track, the intensity being greatest under the wheel and diminishing to nothing on either side. The maximum intensity where the depression is also greatest is $\frac{w}{k}$ tons per lineal foot, this load produces a depression of h feet, and the work done in depressing the track here is $\frac{1}{2}\,h\,\frac{w}{k}$ 2,240 foot-lbs., and the rate of doing work as this depression advances at v feet per second is $\frac{1}{2}\,v\,h\,\frac{w}{k}$ 2,240 foot-lbs. per second. If P is the tractive effort required to provide for this, the work done is P v, hence $P = 2{,}240\,\frac{h}{2\,k}\,w$ lbs., and this will be the total pull required for a train if there is no recovery until the whole train has passed. If the track recovers its normal condition completely before a second axle comes over the same points, the pull as given above must be multiplied by N, the number of axles in the train. Hence the pull in pounds per ton for no recovery is $2{,}240\,\frac{h}{k}\,\frac{1}{N}$ and for complete recovery between each axle is $2{,}240\,\frac{h}{2\,k}$.

Note 2.—Flange-Action.

The principle on which this result depends was outlined by the Author in his contribution[1] to the discussion on Mr. Aspinall's Paper on "Train-Resistance" in 1901.

In *Fig. 12* let $d\,a$ represent one of the rails, and $g\,a$ the wheel-base of

Fig. 12.



a truck in position of mean inclination to the rail, with the leading wheel touching the rail at a, the tread of the rear wheel at g being at a distance $\frac{c}{2}$ from the rail, where c is the maximum play between the flanges and the rails.

The truck tends to roll in the direction $g\,a$ but is prevented from so doing by

[1] Minutes of Proceedings Inst. C.E., vol. cxlvii, p. 225.

the action of the rail on the flange at a, which produces a force P at right angles to the rail acting on the truck and turning it about the point g. The force P will depend upon the moment of inertia of the truck about g.

If the truck is moving with a velocity v it would roll from a to h in unit time if $a\,h = v$. The force P brings the wheel to k over the distance $k\,h = s$ with an acceleration f, where $s = \frac{1}{2}f$. If m is the mass of the truck, and $a\,m$ be the equivalent mass at a for rotation about g, then $P = a\,m\,f = 2\,a\,m\,s = a\,m\,\frac{v\,c}{b}$, and the corresponding tractive effort required is $\mu\,a\,m\,\frac{v\,c}{b}$, where μ is the coefficient of friction. Hence F, the tractive effort per ton, is proportional to $\mu\,a\,\frac{v\,c}{b}$.

The quantity a will depend upon the distribution of the weight of the truck with respect to the wheels, and as this is nearly the same with trucks of different wheel-base, except in unusual cases where there is a very large overhang, this quantity may be taken to be constant. It follows that the tractive effort required to overcome flange-action varies as the first power of the velocity and is proportional to $\frac{v\,c}{b}$.

In the case of a bogie-coach the flange-action is proportional to $m\,\frac{v\,c}{b}$ for each of the bogie-trucks of mass m and wheel-base b. Hence, if M is the mass of the whole coach, including the trucks, the tractive effort per ton is proportional to $\frac{2\,m}{M}\,\frac{v\,c}{b}$.

Discussion.

The President. The President moved a vote of thanks to the Author.

The Author. The Author pointed out, with reference to *Fig. 7*, which gave the results of the Zossen tests, that he had been careful to state in the Paper that the results obtained by the retardation method were carried only to a speed of 75 miles per hour. The Syndicate gave a curve, as shown in the Paper, extending up to 120 miles per hour. He did not mean to indicate by the Paper that the higher part of the curve had been arrived at entirely by deduction. A large number of tests had been made at the higher speeds, but not by the retardation method. He had thought, however, that it would be interesting to give the curve as they gave it throughout the whole length, because the correspondence between that and the calculated curve seemed to be a matter of considerable interest.

Col. Crompton. Colonel R. E. B. Crompton rose more for the purpose of seeking information than with the intention of criticizing the Paper, which was a valuable one to him, as he had already asked himself many of the questions which the Author had attempted to answer. The Paper assembled in a convenient form the results of a number of experimental researches made with a view to determine the various components of the total resistance of a train of vehicles moving along a railway. It might be remembered that last session he attempted[1] to arrive at the air-resistance to vehicles on highways, the present being a motoring age when everybody who travelled in a motor-vehicle experienced for himself the effect of the air-resistance by the pressure of air on the face. Anyone of a reflective mind at once saw that air-resistance must account for a large part of the power which had to be exerted in propelling a vehicle at speeds exceeding 12 miles per hour. He did not propose to make any critical remarks on the advantage or the possibility of separating the total rolling-resistance into rolling-friction and track-resistance; as far as he could see from the Paper, the data obtained were not sufficient to separate them with reasonable accuracy. He would have thought that it was sufficient for all practical purposes to take the two

[1] "Modern Motor-Vehicles." Minutes of Proceedings Inst. C.E., vol. clix, p.2.

together. The Author gave some very interesting facts with regard to air-resistance, and the effect of side wind in particular instances. In certain cases side wind was the cause of the largest component of the whole resistance. In India trains had occasionally been stopped through storms striking them at right angles and forcing the flanges of the wheels against the rails, causing them to grind to such an extent that the train could not proceed. That effect was not uncommon, but was not specially dealt with in the Paper. The Author gave some facts, which Colonel Crompton had long looked for, as to the effect of alteration of the shape of the front and rear of the train. The parabolic cones (Nos. 4 and 5 in *Figs. 1*) were, he presumed, attached to the rear car as well as to the front car, and consequently the suction at the rear of the train was diminished by the shaping as much as the resistance in the front of the train was. He believed those figures were corroborated by experimental work which had been carried out by Colonel Holden and others on projectiles; but he was rather surprised to see the great reduction of resistance shown by the short cones of Nos. 4 and 5. He felt very doubtful whether the figures given in the Paper could be substantiated if further experiments were carried out. With regard to the other element of air-resistance—the skin-friction on the sides and the roof-surfaces which, in the case of trains, was such a large portion of the whole—he did not see that the deduction from Mr. Batcheller's experiments in the pneumatic-despatch tubes of New York, which gave a coefficient of 0·0032 for a machined cast-iron surface, could be taken as final, or even as indicative of the skin-friction of a rapidly moving railway-train. He agreed with the Author that, having regard to the much rougher surface of railway-carriages and the intervals between them, a much higher figure was to be expected; but in spite of his opinion, the Author gave the frictional resistance at only 30 lbs. per 1,000 feet of exposed area at 60 miles per hour. Colonel Crompton agreed that far more experimental work was necessary before the skin-friction of a railway-train could be determined. Then came the highly important question of the air-friction underneath a train, because on that depended largely the amount of dust which was raised both by a railway-train and by a motor-car. The importance of the question lay in the fact that until something more definite was known about the pressures which existed underneath a vehicle moving rapidly over the surface of the ground, little could be done either to reduce or to divert the current of air which was caused thereby, and which played so important a part in raising columns of dust at the rear of a railway- Col. Crompton.

Col. Crompton. train or of a motor-car. Such dust-columns were not seen at the rear of trains in England to a serious extent, owing to the cleanliness of the ballast used; but if the track was at all dusty as it was on some lines abroad—in Spain or in Egypt, for instance—eddying columns of dust were raised at the rear of the train, such as were seen every day on English highways, due to the passage of motor-cars. There was no doubt, however, that the current which resulted from the compression of air underneath a train could be utilized to neutralize the vacuum at the back of the train, and to prevent the dust-column from rising. This had actually been done recently in the case of motor-cars, with excellent effect, the air which passed under the car having been so directed as to neutralize the vacuum at the back, causing the dust raised by the wheels to be deflected horizontally, and to reach the ground-level quickly, whereby the nuisance was largely abated. He thought it was a great pity that the Author expressed the air-resistance in pounds per ton. The air-resistance was usually a question of surface; it had nothing to do with weight, and it should always be stated separately in terms of surface. The experiments with roller-bearings were extremely interesting, but he believed a great deal had yet to be done before roller-bearings could be practically introduced, as the great difficulty was to compel the rollers to keep parallel to the axle. All who had had any experience with roller-bearings knew that they had a tendency to become slightly twisted. So long as the rollers remained parallel to the axle, figures such as the Author had given, showing the resistance to be reduced to one-third, were noted by all investigators; but once the rollers got askew it was found that, after a short time, the resistance crept up to the original figure, and there was no gain.

Mr. Shelford. Mr. FREDERIC SHELFORD pointed out that the formula (No. 6, p. 231) for the pressure of the wind on a surface moving at a certain speed was not in conformity with the results of experiments. He thought the figures given by the Author were wrong in that instance, the pressure at 60 miles per hour being stated to be only 9·14 lbs. per square foot of projected area. He had recently been making some experiments at Brooklands on a motor-car capable of running at 100 miles per hour. Although the experiments were not complete, he could assure the Author that the wind-pressure at 60 miles per hour was more like 18 lbs. per square foot, in fact, he had obtained 18 lbs. at that speed. Nor did he know how the Author had arrived at the constant 0·00254 in the formula, which was very low. Molesworth's pocket-book gave a constant of 0·0049, and Dr. Stanton

in his recent Paper[1] gave 0·0032 for a board 10 feet by 5 feet. Mr. Shelford had obtained at Brooklands much the same figure as that given in Molesworth's pocket-book; and as the air-resistance was spoken of all through the Paper, he thought it must affect the results to a considerable extent. All the resistances dealt with in the Paper were small compared with the power required to haul a train up a gradient or along a curve, and that had to be borne in mind in considering the percentages given by the Author. On p. 228 a description was given of some experiments made on journals, using a pressure of 364 lbs. per square inch. He thought that figure was rather high, as personally he had been using pressures of 250 or 260 lbs. per square inch, and even then had had complaints about the journals. The journals had now been increased so as to give a pressure of 240 lbs. per square inch. The Author's statements about flange-action (p. 230), were, in Mr. Shelford's opinion, not in accord with practical experience. It was stated that the body of a bogie-wagon kept more or less in the centre of the track, and only swayed quietly from side to side, while the bogies oscillated. A ride on the District Railway would probably alter the Author's opinion on that point, because the whole body of the train swayed considerably, and it was well known that the rails on the District Railway were badly cut by that action. He thought wrong conclusions had been drawn from the experiments made on the Northern of France Railway, as shown in the diagrams. The Author had assumed that the air-resistance was the same for four-wheeled stock as for bogie-stock. Mr. Shelford could not understand that assumption, because nobody acquainted with Continental four-wheeled stock, consisting of short cars with large spaces between them, would imagine that the air-resistance for such a train was the same as it was for a bogie-train made up of long cars. In Table II the Author stated that the flange-action of a coach with four wheels, and a wheel-base of 17·7 feet, was more than with bogies having a wheel-base of 8·2 feet. Mr. Shelford could not follow that statement at all. Of course a small bogie was inclined to oscillate from side to side, while a coach with a wheel-base of 17 feet could hardly do so to the same extent. Neither could he follow the formula on p. 238. If the weight of the bogie were kept small, then the flange-action would be reduced considerably. He thought everybody agreed that if the wheel-base of a bogie was short, the flange-action was much increased, apart from the weight. The Author seemed to be a little unfair to railway-engineers in the statements he had made

Mr. Shelford.

[1] *Ante*, p. 175.

Mr. Shelford. with regard to journal-friction on pp. 250 and 251. He mentione a journal-friction, at 60 miles per hour, of 8 per cent., that wa 8 per cent. of the whole resistance on straight and level lin If gradients and curves had to be dealt with, that percentage becan very small, and it was a question whether it was worth while t adopt any special means of diminishing friction in journals in orde to reduce what was already a small percentage. For instance, i Table XII it would have been fairer if the Author had stated th result in terms of the saving in coal or in work done. Th Table really meant that the saving represented 4 lbs. of coal pe train-mile. It was very interesting to know that the London an South-Western Railway had reduced the flange-play to ⅜ inch. H did not know whether any member could say what was th smallest amount that could be used with safety; but he wishe that the South-Western Railway would experiment with ¼ inch.

Mr. Robertson. Mr. F. E. Robertson desired to call attention to a slight verbal obscurity on p. 245, where the Author, in speaking of side wind, gave a figure of 8·7 lbs. per square foot. It should be 8·7 *lbs.* per square foot for a 60-mile wind. The actual wind-pressure in question was only 0·03 lb. per square foot. The question of side wind raised a much more important point in practical train-resistance than appeared from the diagrams. He knew of two stretches of line, one in India and another in Egypt, which experienced at certain times of the year what might be called a pleasant breeze; it was nothing more, but it was broad on the bow, and it made it exceedingly difficult for trains to keep time. A wind blowing straight ahead was fairly met, but a side wind entangled itself in all the excrescences on the coaches, and in the spaces between them; and that was especially the case in India where the coaches were fitted with sunshades. The side wind was therefore a really serious item. He had been in trains which were almost stopped by a side wind pressing the flanges against the leeward rail. It might not be without interest to mention that he could call to mind about twenty instances where trains were upset, both on 5-foot 6-inch and metre-gauge lines in India, and it would need a 25-lb. *to* 35-lb. wind to do that. One case was extraordinary. A metre-gauge train on the Eastern-Bengal Railway was crossing an open-decked girder-bridge, of 40-foot girders or thereabouts, over a small river, when it was caught by the wind and the whole train was upset. Two of the coaches, one of which contained more than a dozen passsengers, were neatly deposited on their wheels in the river-bed. It was clear that the coaches must have turned a complete somersault in order to land on their wheels, or else

they must have been lifted bodily off the rails and allowed to drop. As none of the passengers were injured, he thought the latter was the only conclusion that could be arrived at. Mr. Robertson

Mr. F. Hudleston thought the discussion was wandering from the subject of the Paper, which dealt with ordinary winds and not with heavy gales blowing on the side of a train. They were quite different things. The Author simply called attention to the effect of a head wind and the friction of that wind on the side of the train. Mr. Hudleston believed that most of the experiments referred to had been made in comparatively still weather, with the intention of arriving at the friction which was ordinarily considered by a railway-engineer as the resistance to a train. The Author had made one of the first attempts which Mr. Hudleston had come across to separate all the various resistances which combined to form what was generally called the train-resistance, and had divided them in an extremely ingenious and convenient way. He separated first of all the ordinary journal-friction, practically disregarding the track-friction. All engineers would probably agree that in the case of an ordinary train the track-friction—the effect of the rolling load on the track—was very small, because a large number of axles passed over any given spot, and although the front wheel did not do so, the rest of the train passed over what was practically a dead level, and the resistance due to that cause was small. It was quite different with the single cars which had been used in the electrical tests at St. Louis, and with motor-cars: to those the resistance from the track might be considerable, and the results were not really comparable. What the Author was attempting to determine was the average resistance of a long train of coaches. The results plotted in the diagrams were extremely interesting, those which appealed to him most being the curves deduced from the tests on the Lancashire and Yorkshire and the Northern of France railways. In *Fig. 8* those two lines afforded the middle curves. They were the most valuable of all the tests examined by the Author, and gave a much better idea of the resistance than engineers had been used to. He could not understand the curve for the bogie goods-wagon on the New York, Ontario and Western Railway. Up to 30 miles per hour it was the same as in *Fig. 5*, but, so far as he could make out, it had really been deduced from the results of experiments made at about 19 miles per hour. Beyond that speed the resistance of the air must increase rapidly, and the head wind and side friction must surely bring the curve up more rapidly. It was hardly conceivable that a bogie goods-wagon train, however well made, would have no greater resistance than about 7 lbs. per ton at Mr. Hudleston

Mr. Hudleston. 60 miles per hour. He thought the curve ought to rise in the same way as the other two, and he hoped the Author in his reply would say why that curve was so flat at the end.

Mr. Fowler. Mr. HENRY FOWLER desired, as one of the experimenters on whose tests Mr. Aspinall based his Paper in 1901, to make a few remarks on the interesting Paper before the meeting. The Author referred to the fact of a train virtually running continually uphill, owing to depression of the track by the locomotive, and stated that after the locomotive had passed there was practically no reaction at speeds higher than 10 miles per hour. Mr. Fowler had carried out personally the experiments on that point on the Lancaster and Yorkshire Railway, and although he had not had the opportunity of referring to his notes, he had a distinct recollection of lying down by the side of a main line for one morning and watching every train that passed by; and he did not remember a single instance where there was not recovery in some degree after the engine had passed, and, in the case of a bogie-coach, between the wheels of successive bogies. With regard to what the Author had called flange-action, he desired to point out that Mr. Aspinall in his Paper split up train-resistance in a manner very similar to that shown in *Fig. 4*; only, he did not call this component "flange-action," but "miscellaneous resistances." Mr. Fowler thought the latter was by far the better term, and was probably somewhat nearer the truth. These miscellaneous resistances, like flange-action, varied directly with the velocity, the amount of them being given roughly by the expression $4 \cdot 84\ V - 2$. In addition to the flange-action there were a number of other effects, such as the general oscillation of the train. Even on the best railways in England or the United States, it would be found, when passing along the corridors and through the connecting passages between the various vehicles, that the oscillation of the body of the coach was considerable. Rail-joints also undoubtedly added, even though in a small degree, to the total resistance of the train. With reference to *Fig. 5*, in which two simple resistance-curves were worked out, had the Author any basis for drawing those curves, other than that given in the Paper? There was only one point, from which the whole curve was deduced; and if the total resistance varied as any power of the velocity, it was not fair to build up an entire curve from a single point. With regard to the question of varying weights of trains, he thought the fact might have been overlooked that the figures expressed the resistance in pounds per ton with varying weights. That bore out what he had already said with regard to miscellaneous resistances. The air-resistance of a vehicle did not

vary according to its load; and the miscellaneous resistances did not increase directly with the load. That was why less resistance per ton was obtained with a loaded vehicle than with an empty one. Mr. Robertson had referred to the air striking in between the coaches, and the experiments made on the Lancashire and Yorkshire Railway had shown that this undoubtedly had a marked effect. The air usually came in at both sides of the space between the coaches; it struck the following coach, then rebounded and swept underneath the coach, probably going out underneath farther on. In estimating the resistance due to the bogies underneath a train, he thought the Author's figure, 23·2 lbs. per bogie-truck, was not quite satisfactory, because from the total resistance a number of resistances were deducted, and the remainder was divided by the number of bogies in the train. Many experiments had been made in the endeavour to get some reasonable results by coasting, but he thought that method was satisfactory only where a single coach was dealt with, because, as the Author rightly stated, if there was more than one coach the first coach encountered the whole of the air-resistance, the result being that it was crowded behind. Even with one coach it was rather questionable, when the coach was other than a four-wheeled one, whether this was a satisfactory way of ascertaining a resistance. With regard to Colonel Crompton's objection to the air-resistance being stated in pounds per ton, locomotive-engineers wanted to know what the resistance was in pounds per ton; and although this might not be the best way of putting it scientifically, it was a very convenient one from an engineering standpoint. Mr. Shelford had spoken of wind-pressure being as high as about $0 \cdot 005\ V^2$ per square foot. Mr. Fowler thought it could not be much more than was indicated by the Author's figures and by Mr. Aspinall's Paper, otherwise the air-resistance would exceed the total resistance of the train. Mr. Fowler.

Mr. BERNARD M. JENKIN asked the Author to explain a little more fully *Fig. 8*, where a number of curves were given, apparently for the purpose of comparing the train-resistance per ton for different kinds of vehicles—motor-coaches, bogie-coaches, and goods-wagons. In his opinion those curves were not strictly comparable. The one for the motor-coach was for a single car; the others were for trains, and, if he was not mistaken, in certain cases for trains behind a dynamometer-car, so that the front air-resistance was not taken into account at all. Mr. Jenkin.

Mr. J. S. WARNER thought it was impossible to question the permanent value of the investigations made by the Author. In order to predetermine train-resistance, the train must be taken as Mr. Warner.

Mr. Warner. a whole; but it had occurred to him that it would sometimes be far better to take a separate unit, and to make practically all the investigations, with the exception of that on air-resistance, quite independently of the train as a whole. He had obtained in practice some very successful results by adopting a particular conception which he would endeavour to make clear. The idea was like that of the trajectory of a projectile. Instead of assuming a straight track along which the train ran, the matter was considered from the point of view that a straight line was the line which the body tended to follow, and that relatively to that line, the best possible "straight" track which could be maintained in practice was not straight. He had given the name of "conformity-line" to this line (*Fig. 13*); while for convenience the centre of gravity of a vehicle was taken as the point of the moving mass, which both fixed the locus and formed the reference point to this mathematical straight line of motion "bent" to conformity with general track-direction. The "conformity-line" was the ideal line to be followed by the body of the carriage and the freight for comfortable and efficient transit, and relatively to this line the best possible "straight" track was practically a series of slight reversed curves, and probably more accurate in parallelism than in line. When the problem was approached in that way, a different

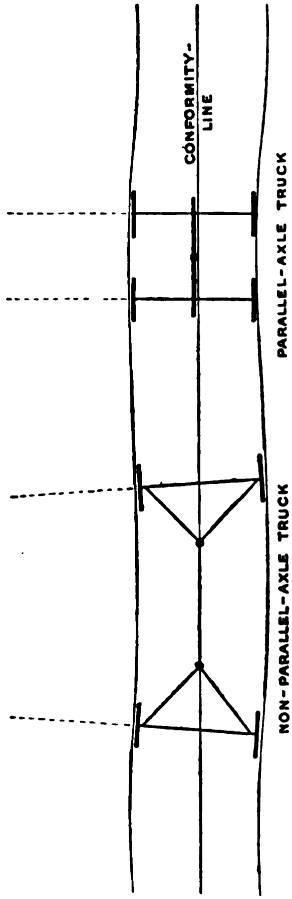


Fig. 13.

conception was formed of what was required when a train or an independent vehicle was run over rails. The Author referred in the Paper to the displacement of the body relatively to the track, and one of the speakers had mentioned the same thing in connection with the District Railway. There was no doubt whatever that the centre of gravity of a coach was practically never exactly over

the centre of the track for more than an instant: the coach was floating from side to side the whole time. Another speaker had referred to the oscillations which occurred in trains of carriages. That was not to be wondered at when it was considered that an attempt was being made to run in a straight line over rails which were not straight. The difference between the conformity-line and the lines of the two rails was much more important, as was found from practical experiments, than was generally considered to be the case; and it was therefore to be expected that the vehicles always would oscillate unless specifically designed to follow the conformity-line. In spite of the elementary fact, of which all engineers were aware in the abstract, that lines of rails were not nearly so straight as the mathematical line of motion, examination of any existing rolling stock revealed in several ways the assumption that the straightness of the two rails was good enough. He had discussed the conformity-line with a number of traction-engineers in England and abroad, and not one had previously employed such a conception; while it was readily admitted to be the only correct method of approaching the problems of carriage-suspension and flange-steering. He had suspended single self-propelled units and run them over the best straight track which he could obtain, and by the device known as the Warner non-parallel-axle truck, which was in regular service on a number of lines in England, had obtained a vehicle which ran with an almost total absence of torsional disturbance, or of swaying or oscillation. He would not describe the method by which it was done, but the fact remained that it was possible to do it; and he thought that by due attention to the conformity-line the upper curve of *Fig. 5* (p. 244), with the racking of track and wagons, noise, oscillation, and excessive draw-bar pull which it represented, together with fuel-consumption, could be very considerably reduced. The same applied to the bogie-wagon or lower curve in *Fig. 5*. Mr. Warner.

Mr. H. Kelway Bamber observed that, as he had had charge for some years of the rolling stock on the East Indian Railway, a few remarks upon his experience might be useful. With regard to journal-friction, the Paper would be welcomed by the carriage-superintendents in India, so far as coaching stock was concerned, because the pressures mentioned in it were about the usual pressures in India for such stock. But as regarded goods-vehicles, in which, Mr. Shelford would be surprised to hear, the pressure was as high as 750 lbs. per square inch, the matter was totally different. The figures in the Paper were apparently based upon some experiments made years ago with pressures of about 360 lbs. per square Mr Bamber.

Mr. Bamber. inch. So long as the gross tonnage of the wagons in India stood at 22 tons, very little trouble was experienced in getting journals to run cool; but as soon as it increased to 28 tons hot boxes became common. On going carefully into the matter he found that at about 550 lbs. per square inch gun-metal was doing as much as it could with any expectation of cold running. For that reason, at his suggestion, the use of anti-friction metal, combined with cold rolling, had been adopted for the journals on the goods-stock. He wished to emphasize the point that, when pressures so high as 600 lbs. per square inch were obtained, the journal itself became compressed, much in the same manner as india-rubber did when a pencil was rolled over it. He attributed the freedom from hot boxes which had been secured since the adoption of cold rolling to the fact that the journal was subjected initially to a stress about ten times as high as it would ever be subsequently called upon to withstand. One of his most trying experiences with hot boxes occurred with the train in which Lady Curzon travelled on her return to India after her serious illness in England. That train suffered from as many as eight hot boxes. For many months Lord Curzon had used it without trouble; but when the journey to Bombay was made to meet Lady Curzon, Mr. Bamber was asked to forgo the repacking of the boxes at the reversing-station. In a weak moment he gave way, with the result that within 26 miles of the reversing-station at Allahabad trouble arose. It was due to the piling of the cotton-waste in the axle-box, owing to the friction of the journal against it when the journal began to get warm. The mail-trains were run for 1,200 miles at an average speed of 40 miles per hour without any trouble with the boxes; but if the boxes were opened at the end of a run it would be found that the cotton-waste which had been squeezed into them had disappeared from one side of the box and had climbed up on the other, although it would have been thought impossible to move it; and unless it was repacked, lubrication ceased as soon as the journal began to revolve in the opposite direction. That difficulty had been entirely overcome by the use of spring pads, known as Armstrong oilers. With reference to flange-resistance, he could not quite follow the Author when he said that only the weight of the bogie-trucks had to be taken into account in a bogie-vehicle. In designing a train for the use of the Prince and Princess of Wales in India, Mr. Bamber decided to depart from the usual practice of putting the weight of the coaches on the pivot, and put it entirely on the sides, on roller-bearings. The coaches weighed 45 tons each,

Mr. Bamber. and he could not but believe that the weight of the coaches had some effect in steadying the bogie. The coaches measured 72 feet over the buffers, the bogie-trucks were arranged on four bearings per truck with a 13-foot wheel-base, and the train ran so steadily at 60 miles per hour that a liqueur glass could be filled and put on the table without spilling a drop. That was not his experience in England, even on some of the best lines. As Mr. Robertson had remarked, wind-pressure in India was an important factor in train-resistance, chiefly because of the use of sunshades on the trains, the wind thus having a much larger surface to rub against. He ventured about 5 years ago to discard sunshades altogether, and to use for the sides materials of highly non-conductive nature. That practice had enabled the coaching stock of India to be increased in length from 56 feet to 72 feet, with attendant advantages as regarded paying load. When designing the royal train he at first proposed a length of 56 feet or 60 feet for each body; but when Lord Curzon's requirements necessitated 72 feet for each vehicle he was in a difficulty. He found, however, that by discarding the sunshades he could gain 6 inches on either side of the vehicle, and could extend the vehicle to 72 feet in length without the side movement through tunnels and on curves being any greater than it was for the 56-foot coach. Although he had not had the opportunity of preparing any diagrams, such as those given in the Paper, to show the effect of discarding the sunshades, he had gathered from the drivers that it was entirely satisfactory in lightening the load on the engine draw-bar.

Mr. Gadsby. Mr. C. H. Gadsby thought that the Author, in his classification of losses, had omitted some of the smaller items which should not be altogether overlooked. For instance, no mention was made of some of the internal losses in rolling stock, such as the axle-box losses in friction in the guides, the friction of draw-bars in the beams, the friction between the laminations of springs, the friction of buffer-stems in their sockets, and of the bogie-centres and bolsters underneath the coaches. Those were all in themselves small matters, but he thought they should be included under some general head; and for that reason it might have been better if the Author had added a "miscellaneous resistance" loss as Mr. Aspinall had done. The journal-friction was an item which offered less ground for doubt than most of the other items included in the formula. He did not see mentioned in the Paper, however, the fact that the weight of the wheels and axles should be deducted before arriving at the total resistance due to the journal-friction. The Author had adopted the

Mr. Gadsby. coefficient of 0·0077 for journals. From very careful study of Mr. Beauchamp Tower's Paper Mr. Gadsby had come to the conclusion that that was a little low, and he had always used the coefficient 0·01. He had no doubt the general conclusion given by the Author as to the rolling-friction was correct, but the data upon which it was based seemed to be extremely meagre. Professor Osborne Reynolds's results, as far as he could see, were based on experiments with steel and other rollers on steel and other plates. He suggested that a better way of investigating rolling-resistance would be by rolling wheels and axles upon slightly inclined rails, and computing the resistance from the gravity effect.

The Author. The AUTHOR, in reply, observed that while Colonel Crompton suggested that it might be sufficient to take track-resistance and rolling-friction together, making no attempt to distinguish between them, Mr. Hudleston had pointed out that in the case of single cars it was important to consider track-resistance as apart from rolling-friction, but that in the case of long trains track-resistance, measured in pounds per ton, might be safely neglected. Further tests were no doubt desirable, to determine at what speed the depression became permanent. The coefficient of friction for air-resistance against the sides of trains had been determined not from the experiments on the pneumatic-despatch tubes but from the tests on the Northern Railway of France, which gave 79 lbs. per 1,000 square feet of exposed area at 60 miles per hour, as compared with 30 lbs. observed in the tubes referred to. With regard to Colonel Crompton's objection to the computation of air-resistance in pounds per ton, all the calculations in the Paper gave the actual resistance, the reduction to pounds per ton being effected in order to render possible an estimate of the relation of air-resistance to the other resistances, and also to enable the whole resistance to be simply expressed. Mr. Fowler had rightly pointed out that this was the only practicable method from the railway-engineer's point of view. The use of roller-bearings in ordinary train-service must of course depend upon their wearing capabilities: on this the Paper expressed no opinion, the object being only to make clear the position as regarded the saving of energy. In regard to Mr. Shelford's remarks on the coefficient 0·00254, the value given in Molesworth's pocket-book was the coefficient that would be used in ascertaining the force of an isolated jet of air on an indefinitely extended plane, and was derived from the equation $P = \frac{Gv^2}{g}$, where G was the weight, in pounds, of a cubic foot of air. This equation was inapplicable to the case under discussion.

The Author.

The coefficient given in equation 6 (p. 231) represented the force on a small flat plane placed in a current of air, the force due to the velocity of the air being given by the equation $P = \frac{Gv^2}{2g}$. This was the pressure that would be observed in a Pitot tube, and it corresponded with the pressure exerted on the centre of the windward side of a large plane exposed to a current of air directed at right angles to its surface. If G were taken at 0·0763 lb., and the velocity in miles per hour, this equation became $P = 0 \cdot 00254\ V^2$ lbs. per square foot. The total force on the windward side of the plane involved the use of a slightly smaller coefficient, owing to the reduction of pressure near the edges; but the suction at the leeward side added greatly to the whole force on the plane, raising the coefficient to the value observed by Dr. Stanton for a plane 5 feet square, namely 0·0032. The case of a train moving with a known velocity in still air differed from that of a flat plane in two important respects. In the first place the rear suction was not nearly so large in proportion to the front pressure, and in the second place the ground introduced a disturbing effect. In the case of flat plates or planes the rear suction might be as much as 65 per cent. of the front pressure, whereas experiments had demonstrated that in the case of a single car, such as that tested at St. Louis with a standard profile, the rear suction was about 30 per cent. of the front pressure; the latter however, owing to the shape, being only 55 per cent. of that experienced by a flat-ended car. The result was that the combined force on front and rear was only 65 per cent. of that due to the velocity as given by equation 6. These were the considerations which accounted for the air-pressures on a train being so much less than those observed on flat plates. The high values noted by Mr. Shelford in his experiments at Brooklands were no doubt due to the fact that he was observing the pressure on a screen placed in front of the car; such observations had no bearing on the case of air-pressure on a train. No assumption had been made in the Paper as to the relative air-resistance of the two types of trains tested by Mr. Barbier. The fact that the resistances were practically the same was proved conclusively by the form of the curves. The actual resistance of the bogie-trains, as shown by Table III, when estimated in pounds, was rather greater than that of the trains of four-wheeled coaches; but when estimated in pounds per ton they were the same. The method of comparison between ordinary bearings and roller-bearings adopted in Table XII was not in any way due to the Author, but was identical with that followed by the Eastern Bengal State Railway in the consideration of this question.

The Author. Mr. Shelford had referred to the swaying of the carriages on the District Railway when disputing the accuracy of the Author's statement that the body of a carriage remained "more or less in the centre of the track, or subject only to slow and gradual side sway." The Author was well aware of the conditions of running on this railway, and would draw attention to the fact that, while the District Railway Company's coaches did sway in the manner referred to, those of the Metropolitan Railway Company, when travelling over precisely the same track, hardly swayed at all and conformed to the action described by the Author: showing that the swaying was due to the inferior manner of suspension employed on the trucks. The Author had attempted to deal in the Paper with rolling stock of the character generally found on the leading British and Continental railways, and he would maintain that his description of the motion of the body of a coach was very fairly applicable to rolling stock of that character. In speaking of flange-action Mr. Shelford had apparently omitted to observe that the resistance caused thereby depended upon the wheel-base as well as upon the weight of the part that oscillated, and that it was only by considering the two factors together that correct conclusions could be reached. In reply to Mr. Hudleston's question as to the form of the curve for bogie goods-wagons given in *Fig. 8*, the air-resistance for these wagons had been computed for all speeds up to 60 miles per hour, and the results were given in detail in the paragraphs dealing with these wagons. The reason why the curve appeared to be so flat at the high speeds was because the air-resistance at 60 miles per hour was only 0·3 lb. per ton, as compared with 5·7 lbs. for the Northern of France coaches. Air-resistance was, in fact, an almost negligible quantity, and in consequence the curve became almost a straight line. The experiments described by Mr. Fowler, on the depression of the track caused by a passing train, were of great interest. The speed at which there was no appreciable recovery was no doubt ill-defined and difficult to determine, but the evidence generally seemed to show that, for speeds above 10 miles per hour, track-resistance on a first-class permanent way in good order might safely be neglected in comparison with the other elements of resistance. Mr. Fowler preferred to retain the term "miscellaneous resistances" to include flange-action, general oscillation, rail-joint effect and other unascertained resistances. The Author had endeavoured to put an end to the vagueness and uncertainty that such a term involved. He was, of course, aware that there were other elements of resistance such as those referred to by Mr. Fowler, but he was of opinion that these had been shown to be negligible compared with flange-resistance. It

The Author.

was of the utmost importance to be able, if possible, to predetermine train-resistance; in order to do this the factors to be dealt with must be known definitely, and predetermination would be rendered impracticable if the influence of a number of resistances about which nothing definite was known, and whose effect on the net result did not amount to 5 per cent., had to be considered. The same remark applied to the resistances cited by Mr. Gadsby, such as friction of draw-bars, buffer-stems, springs, and the like. Mr. Fowler had suggested that each of the curves in *Fig. 5* was based on a single observation only. These curves, like the others given in the Paper, had been computed from the formulas with the aid of known data. The circles shown in the figure indicated the results of tests. In this case two circles only were shown, from which it might be supposed that the comparison with the results of calculation was less complete than was the case, say, in *Fig. 4*. A reference to the Paper would show that the circles in *Fig. 5* did not represent isolated readings on the dynamometer, but the results of an extended run over a considerable distance, in which the means of the continuously-recorded speed and dynamometer-readings were taken, after correcting for difference of levels between start and finish. This was probably the most accurate method of testing for train-resistance, and although only one point was finally ascertained, the result, for the purpose of comparison with calculation, was probably better than if a number of points at different speeds had been obtained with the dynamometer. Replying to Mr. Jenkin's question as to *Fig. 8*, the curves there shown gave the tractive resistance behind the draw-bar of the locomotive except in the case of the St. Louis electric motor-coaches, and it was quite true that, from the point of view of comparing the resistances of trains composed of different types of rolling stock, *Fig. 8* was open to the criticism raised, namely, that three of the curves did not include the front air-resistance. The Author, however, had no wish to constitute a comparison of this kind. His object was two-fold: first, to show on one sheet the different curves that had been predetermined, in order to draw attention to the great variation between them and to emphasize the fact that it was possible to predetermine resistances which did so vary; secondly, to demonstrate the impracticability of attempting to deal with train-resistance by any method which neglected to take account of the type of rolling stock under test, and the futility of the endeavour to find a single formula which should apply equally in all cases. Mr. Bamber's remarks as to journal-pressure were of considerable interest and practical value. The pressure of 750 lbs. per square

The Author. inch which he mentioned was not greatly in excess of the highest value quoted in the Paper, namely 582 lbs., and there did not appear to be any reason to think that the results of the experiments quoted would have been materially altered if the pressure had been increased to the higher value. The Author had aimed at obtaining a value for journal-friction that would represent ordinary railway-practice, and he thought that although in special cases a value of 750 lbs. might be reached, the figure of 600 lbs. per square inch, to which Mr. Bamber's remarks more particularly referred, was a more practical working-limit to adopt, and this was about the limit chosen in the experiments. With reference to the tendency of the weight of the body of a coach to steady the swaying motion of the bogies, there was no doubt that side-bearing bogies swayed less than those with a centre bearing; but although this gave a certain steadying effect, it might be doubted whether it reduced the flange-resistance. The Author's experience was that side-bearing bogies tended to remain hard over in the direction determined by the last curve that had been passed, and that the consequent grinding of the flanges on the rails was, if anything, greater than when centre bearings were used.

Correspondence.

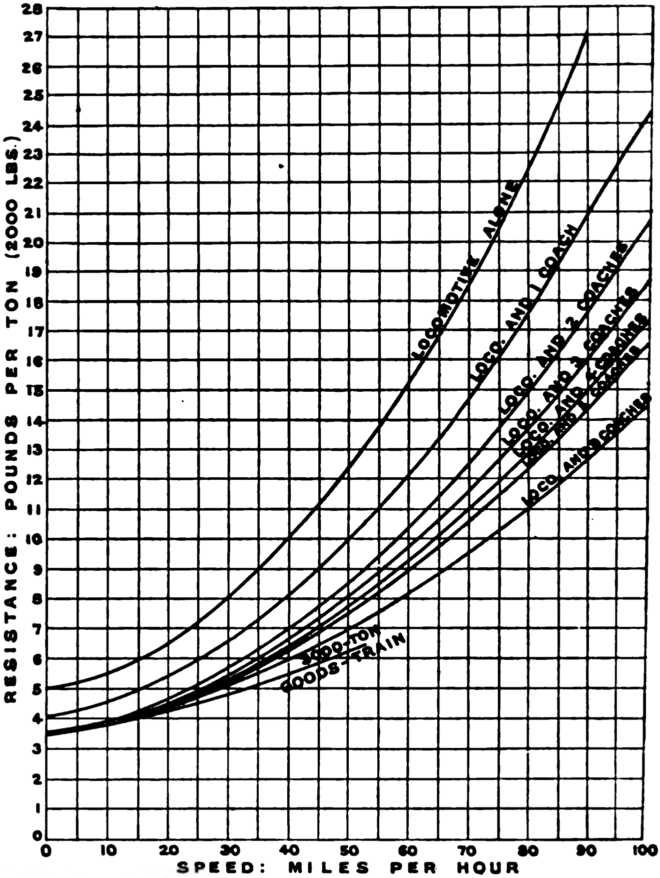
Mr. Armstrong. Mr. A. H. Armstrong, of Schenectady, offered for consideration various data which had at least the merit of laying the foundation for a workable train-resistance formula. Lest he might be accused of attempting to create a place for a new formula in a field already stocked with such creations, he would state that it was of the three-part type advocated by the Author, which form seemed to be generally accepted as best fulfilling the required conditions. The resistance opposing the forward motion of a moving train depended upon such complex relations between weight and speed, and car-, truck- and track-construction as to render it impossible to accomplish more than general grouping of the several elemental resistances, with the result of producing a formula giving approximate values only. As a formula, to be of any value, must be capable of wide application, it would appear that values of A in the Author's general equation (p. 233) could not be made dependent upon such factors as journal- and wheel-diameters, knowledge of which was not always available. In order to suit A in various weights and aggregations of cars, he

had found it convenient to use $A = 50 \sqrt{T}$, where T was the total weight of the car or train in tons (2,000 lbs.). This value was purely arbitrary, but was chosen on account of its close agreement with results obtained from many experimental tests on dissimilar vehicles: not that the term A was determined independently from the tests, but that that term expressed as above and used in conjunction with the rest of the formula to be given later, yielded results closely approximating to test values, over a wide range of types of equipment and of speed. The Author suggested that the well-known difference in tractive effort required to haul a loaded and an empty vehicle might be attributed to B, the coefficient of v in the second term; but by using the expression $A = 50 \sqrt{T}$ this difference in tractive effort between loaded and empty cars was taken care of appropriately in the first term, and might rightfully belong in this term if lubrication of the journal was somewhat more complete than was assumed by the Author in making the statement that the coefficient of journal-friction was constant at all pressures. Mr. Armstrong's experience in determining train-resistance had been limited chiefly to tests in which electrical methods of measurements were made use of: such methods presented a ready means of checking results obtained by coasting. Probably one of the most complete sets of tests taken for the express purpose of determining the resistance to trains of different composition was that undertaken upon the experimental track of the New York Central and Hudson River Railroad during the 50,000-mile endurance-trial of the new 100-ton electric locomotives. These tests, comprising many hundred runs with trains of different composition, under different climatic and atmospheric conditions, had given very valuable data, from which the curves of *Fig. 14* had been plotted. Together with similar tests upon electric motor-cars of several types, they established the fact that the air-resistance constituted the most important element of the total resistance with light trains, and was a considerable factor even in the case of a nine-coach train weighing, complete with locomotive, approximately 550 tons (of 2,000 lbs.). The fact thus established led him to put greater faith in train-resistance measurements where electrical methods were used, and, for reasons given later (p. 287), to place but little reliance on results obtained with either the dynamometer-car or with steam-indicator diagrams, and especially to discredit figures deduced from water- and coal-consumption over an extended run. Most of the records of the electrical runs made in the New York Central and other tests had been obtained by the coasting method, continuous records being kept of speed, time,

Mr.Armstrong.

Mr. Armstrong. distance, etc., but these figures had been checked by meter-readings of voltage and current-input to the motors, also continuously recorded. As the New York Central locomotives were without gears, this troublesome factor was eliminated. The coasting method, checked by the electrical records, also gave opportunity of determining

Fig. 14.



both the constants B and C, by taking advantage of the fact that the coefficient of the second term varied directly as the speed, while the coefficient of the third term varied as the square of the speed. Instead, therefore, of making all tests upon days when there was little or no wind, tests had been purposely made upon days when the velocity of the wind in the direction of the track reached 20 miles

per hour or more. Thus, a train running at 50 miles per hour with the wind encountered a head wind of 30 miles per hour, while running against the same wind at a speed of 50 miles per hour was equivalent to meeting a head wind of 70 miles per hour. In each case the actual speed of the train in relation to the track was 50 miles per hour, and B remained constant, assuming of course that the wind was directly head on, and did not increase the flange-friction. The difference in train-friction recorded at 50 miles per hour with and against a wind of 20 miles per hour must necessarily correspond with the difference in the third term, and in this case the coefficient C could be obtained directly. In the same way, the coefficient B of the second term could be obtained with a wind-velocity of, say, 20 miles per hour in the direction of the track by running the train at a speed of, say, 70 miles per hour with the wind and 30 miles per hour against the wind, the actual head wind opposing the motion of the train being in each case 50 miles per hour, thus constituting equal values for the third term. By following out this method of making resistance-tests, and taking records on days when the wind was quartering to the track, it was possible to study fully the coefficient B under all conditions, and to determine the value of flange-friction under normal conditions and with a cross wind—a matter of considerable importance. It was unfortunate that lack of time had prevented the full carrying out of the schedule of tests contemplated on these lines, and that only partial results had been finally obtained. Sufficient data had been obtained, however, to convince Mr. Armstrong that the widely scattered points usually observed in the plotted results of train-resistance tests would be brought more nearly in line with the aid of a full knowledge of the atmospheric conditions obtaining (direction of the wind, its intensity, etc.), not only in general during the test, but also as determined by a continuous record made both on the train and at frequent stationary points along the direction of travel. This applied more especially to light trains, in respect of which the air-resistance was of vital importance. Owing to the impossibility of applying train-resistance data obtained from dynamometer-tests to the conditions obtaining with the single car or small aggregation of cars common in electrically propelled trains, he had been forced to build up a formula that would fit the conditions with which an electrical engineer was more especially concerned, with the result that, using the recognized three-part formula advocated by the Author, the following (*Fig. 14*) had been found to apply very closely to working-conditions— Mr. Armstrong.

$$R = \frac{50}{\sqrt{T}} + 0\cdot03\,V + \frac{0\cdot002\,V^2}{T}\,A\left(1 + \frac{N-1}{10}\right).$$

Mr. Armstrong. where R = resistance in pounds per ton,
T = total weight in tons,
V = speed in miles per hour,
A = end cross section in square feet,
N = number of cars,

and $50\sqrt{T}$ was limited to a minimum value of 3·5. The following values were assumed for A and T:—

	A	T
Locomotive	120	100
Passenger coaches	100	50
Goods-wagons	90	40

It was interesting to note the agreement between test results and the formula, as shown by the following figures:—

New York Central Locomotive Alone.

Speed (miles per hour)	20·0	40·0	60·0	80·0
Calculated resistance (lbs. per ton)	6·55	10·0	16·2	22·5
Test resistance (lbs. per ton)	6·7	9·6	14·0	17·5

New York Central Locomotive and Five Cars.

Speed (miles per hour)	20·0	40·0	60·0	80·0
Calculated resistance (lbs. per ton)	4·5	6·31	8·92	12·32
Test resistance (lbs. per ton)	4·2	6·85	9·55	12·40

New York Central Locomotive and Nine Cars.

Speed (miles per hour)	20·0	40·0	60·0	80·0
Calculated resistance (lbs. per ton)	4·44	6·07	8·4	11·4
Test resistance (lbs. per ton)	4·0	6·5	9·1	11·6

Allgemeine Car in Zossen Tests.

Speed (miles per hour)	20·0	60·0	80·0	100·0	120·0
Calculated resistance (lbs. per ton)	5·6	14·7	22·2	32·0	43·3
Test resistance (lbs. per ton)	5·5	14·9	22·8	33·3	46·0

Car No. 5, General Electric Co.

Speed (miles per hour)	10·0	20·0	40·0	60·0
Calculated resistance (lbs. per ton)	8·4	10·6	18·4	31·5
Test resistance (lbs. per ton)	8·3	11·3	19·2	29·2

The suggested formula was not in any way complete, and its several elements could readily be subdivided to advantage; noticeably the first term, $50\sqrt{T}$, should be subdivided into a constant for all trains plus a variable depending upon the composition of the train, its weight, etc., in order to obviate the necessity of limiting $50\sqrt{T}$ to a minimum value of 3·5. The last term also was not accurate, in that it did not differentiate between the head-on wind-friction and that of the sides and top, this error being due to a

desire to provide a simple formula. The subdivision of coefficient C into several parts to be calculated separately, as suggested by the Author, was undoubtedly more nearly in accord with actual facts. The formula suggested by Mr. Armstrong depended for its success upon the relations generally existing between the length of a car and its weight as obtaining in the United States, and the formula would not apply with equal accuracy to the much shorter, lighter cars used in England and on the Continent. His grounds for saying that he considered train-resistance data obtained from dynamometer-car or steam-indicator records to be unsatisfactory, were the following:—The tractive effort recorded by the dynamometer-car was the effort required to haul the load trailing behind the locomotive and tender, and with light trains this tractive effort might be perhaps but one-half or even less of the total tractive effort exerted by the locomotive. Both the steam-engineer and the electrical engineer had common interest in determining the total tractive effort required to haul the train as a unit, and while the error introduced was small in the case of goods-trains, the results obtained were entirely misleading in the case of high-speed passenger-trains, even though these were of considerable weight. Further, dynamometer-records taken over extended runs were open to serious criticism, due to the fact that the resistance could not be obtained from such records by deducting or adding the tractive effort due to difference in elevation, as a continuous dynamometer-record included all values of curve-resistance, and acceleration of the train at the original start, as well as after rounding curves where it might be necessary to reduce speed; but more especially it included part of the tractive effort due to the difference in elevation of the two ends of the run, unless the intermediate gradients were so slight that they could be descended without application of the brakes. The energy lost in braking must be replaced, and would be recorded in the dynamometer-readings, so that any continuous test to be of value must be made upon practically straight level track. In addition to the inaccuracies inherent to a continuous run, the method of determining train-resistance by coal- and water-consumption was open to the further criticism that such methods of measurement were, at best, very crude compared with the coasting method or determination of train-resistance by the input to an electrically propelled train. Further, the coal- and water-records included not only the fuel and steam consumed in useful work, but also the losses introduced when the locomotive was coasting downhill and doing no work, but still consuming coal at a rate high enough to invalidate the accuracy of the results. The coal- and water-consumption were of interest

Mr. Armstrong. to the steam-engineer, but they did not afford an accurate means of determining train-resistance when considered from the standpoint of another type of motive power. In general, all tests which had come under his attention had demonstrated the great importance of air-resistance as a factor in the total train-resistance, and while attempts were being made to shape the front end of motor-cars operated singly so as to reduce train-resistance, steps were also being taken to reduce the skin-friction by introducing cars of smooth exterior design, a notable example being the all-steel car designed for the Union Pacific Railroad Company, following the lines of their gasolene motor-car. He desired to compliment the Author upon his clear method of subdividing the train-resistance into its various components, and upon offering the best presentation of the subject in this respect that had yet been published. Mr. Armstrong felt that, his experience having been limited to light trains running at considerable speeds, his remarks should be interpreted as applying more especially to such trains.

Mr. Cardew. Mr. C. E. Cardew remarked that he had done much experimental work with the Timmis bogie-lead on the Burma railways between 1900 and 1905, in communication with the late Mr. I. A. Timmis, M. Inst. C.E., its originator. Mr. Cardew early came to the conclusion that in certain cases its action in steadying a bogie-vehicle by preventing radial oscillation of its trucks around their pivots—generally known as "hunting"—was distinctly beneficial, especially in short bogie-vehicles descending long inclines, when the hunting action of the trucks was always very marked. In the discussion on Mr. Aspinall's Paper[1] Mr. Timmis communicated some of the results of the earlier experiments to The Institution. Besides the radial oscillation of the bogie-trucks, there was a transverse oscillation of the body of the vehicle on the swinging suspension-links of the trucks. To this motion—usually termed "lurching"—the Author did not refer. In many cases it appeared to be indirectly due to the hunting of the bogie-trucks, and in short vehicles it might be very marked and disagreeable to passengers. Under the term "lurch" was not to be included the gentle rhythmic swaying of the vehicle, due to the pendulum action of the swing-links, which in itself was not disagreeable, but only those violent and irregular oscillations which were set up periodically from shocks received by the wheel-flanges impinging on the rails. When the hunting of the bogie-trucks on the track was stopped, the lurching of the body was obviated or at least reduced to a

[1] Minutes of Proceedings Inst. C.E., vol. cxlvii, p. 212.

minimum. Mr. Cardew had found that this desirable result was effected by giving lead to the trucks. To prove the value of the relief thus afforded he had made an experimental arrangement in a car whereby it was possible to apply or remove the lead at pleasure. On running this car at the end of a train without connecting the continuous brake to it, so as to leave the trucks and wheels quite free, it was found that the difference between coasting downhill with and without bogie-lead was very marked. In order to prevent objectionable lurching, it was a common practice to employ side-resistance springs, either in substitution for or in combination with the swing-links; but they were generally more of a palliative than a cure for the trouble. From long experience in the designing, building, and running of rolling stock he was convinced that the longer the vehicle the less the hunting of the bogie-trucks and the lurching of the body, so that generally both were of little or no practical importance in very long vehicles. He was therefore surprised that the Author omitted the length between the centres of bogie-pivots as a factor in the equations he employed in dealing with the question of oscillation. On p. 259, however, this factor was admitted as one to be considered, where the Author treated the action of bogie-lead as equivalent to giving a bogie-vehicle a temporary rigid wheel-base equal to the distance between bogie-pivots, that was, of course, while the vehicle was running on a straight track. Mr. Cardew, however, held that this came about automatically as a function of the length between bogie-pivots, quite independently of any lead given to the trucks. If so, then the equations needed modification to include and express this function. Further, the objectionable oscillations were greatly magnified on poor track and at high speeds; indeed, they could scarcely be studied advantageously except under those unfavourable conditions, and it was exactly under such conditions that the advantage of bogie-lead made itself most felt. He agreed with the Author, however, in condemning excessive side-play between wheel-flanges and rails on straight track. Were that reduced, there would be much less complaint about hunting and lurching, and probably special devices for overcoming these irregular motions would be less necessary. The Author mentioned the tendency of the vehicles in a train to crowd together when "coasting" a descending gradient. He might, however, have added that it was only during this crowding that the objectionable oscillations occurred, that was to say, during periods when the pull on the draw-bars (or central couplers) and on the bogie-pivots ceased, while the transverse stiffness or rigidity of the buffers (or couplers) was the only steadying force on the vehicles composing the train. This Mr. Cardew.

Mr. Cardew. steadying action of the draw-bars and buffers could indeed only b appreciated and studied in perfection on loosely-coupled trair descending long inclines under the control of only engine- and tendeı brakes. With tightly coupled trains fitted throughout wit continuous brakes the oscillations were largely, though not wholl; extinguished. It would therefore be interesting if the Author coul say under what conditions the oscillograms shown on p. 295 had bee taken, the state of the track, and whether the gradient was up or dowr To thoroughly elucidate mathematically the oscillations of the vehicle of a train crowding each other when coasting downhill, and the flange resistance to which such oscillations gave rise, it was necessary t regard the train as a long flexible column in unstable equilibrium The couplings of the vehicles, being then in compression, were s many points at which buckling of the column might take place. In ascending an incline, however, the couplings being all in tension the column was of course in stable equilibrium, with no tendency to buckle, which greatly reduced oscillation and therefore flange-resistance. Fortunately this occurred just when there was an increased demand for power on the motor hauling the train. On the other hand, though the increased flange-resistance in coasting downhill might make no increased demand on tractive power, yet it certainly conduced to much needless wear of rails, wheels, and other running-gear of vehicles, so that every improvement in design of rolling stock calculated to diminish the mischief was to be welcomed. In order to effect this with certainty, however, it was first necessary to comprehend thoroughly the mathematical conditions of the problem. It was to be hoped, therefore, that the Author might be induced to supplement his admirable Paper by giving for flange-resistance some revised equations, in which might be introduced three new factors, namely (1) Distance between centres of bogie-pivots; (2) Total length over buffers (or central couplers); (3) The coefficient of transverse stiffness or rigidity of couplings while coasting on straight track.

Mr. Collinson. Mr. ARTHUR COLLINSON considered that there were several factors which would prevent the general adoption of roller-bearings as at present designed; among them were high first cost, extra weight, liability to get out of order, difficulty and high cost of repair, and, as regarded their adoption for wagons, the necessity for perfect alignment, which was seldom obtained where wagons were built at the rate of one hundred per week, and were anything but a finished piece of engineering work. Roller-gear must be carefully fitted to the wagon, the wagon must be built square and kept square, and the whole vehicle must be made a really good job. The

Spencer roller-bearing had so far been very successful in England, and a large number of high-capacity wagons were running well with this gear, which he understood had lately been further improved. Mr. Collinson.

Mr. C. O. Mailloux, of New York, observed that if the figures and facts which the Author had collected were admitted, discussion must be confined largely to the manner in which these figures and facts were classified and analysed by him. Here there was much room for divergence of opinion, and it need not seem strange, therefore, that Mr. Mailloux should differ radically from the Author in regard to the classification of the elements of train-resistance. While he did not see them, Mr. Mailloux would not deny that the proposed classification might have advantages practically. He was of opinion, however, that this classification could scarcely be considered adequate and satisfactory for the purpose of a scientific analysis, from which a comprehensive formula, or a general equation, was to be deduced. For instance, flange-friction was a particular case of sliding-friction, as was journal-friction. It was usually of the "unlubricated" variety, but, not infrequently at curves when the rails were greased, flange-friction became a case of "lubricated" sliding-friction, precisely like journal-friction. Mr. Mailloux had elsewhere[1] expressed the opinion that a classification of the elements of train-resistance such as that adopted by Mr. Aspinall and other investigators, while perhaps suitable enough for practical purposes, in calculating and comparing train-resistances, was not the most suitable or convenient for the study and analysis of train-resistance, or the determination of its component parts and of the rôle and effect of each. A clearer and more comprehensive idea of these effects was likely to be gained by reference to the fundamental laws of friction. If the components were grouped under the three principal kinds of friction distinguished by the physicist, namely, sliding-friction, rolling-friction, and fluid-friction, the classification on p. 292 was obtained. This complete classification was the most convenient one known to Mr. Mailloux. In devising a formula, it might be convenient or necessary to group together several of these elements; but even then the classification given by Mr. Aspinall was much to be preferred to that given by the Author. The following classification merged the second and third elements of the Author's classification, because they were really different phases of rolling-resistance, and, in the present state of knowledge, could not be separately determined, as the Author admitted. His statement that rolling-friction and track-resistance had not been determined by test, Mr. Mailloux.

[1] *Harvard Engineering Journal*, vol. ii, No. 4 (1904), p. 239.

U 2

Mr. Mailloux.

A.—Sliding-Friction.

(Including two varieties, both involved in train-resistance.)

I. *Lubricated Sliding-Friction*:—
  (1) Rotational friction of axle or journal.
  (2) End-play friction of axle or journal.

II. *Unlubricated Sliding-Friction*:—
  (3) Slipping- or skidding-friction.
  (4) Wheel-flange friction.

B.—Rolling-Friction.

(1) Friction due to mangling or crushing effects.
(2) Friction due to non-yielding inequalities of surface.
(3) Track hysteresis.

A.B.—Composite Friction.

(Combining sliding- and rolling-friction.)

(1) Effects of oscillation and concussion.
(2) Effects of curves.

C.—Fluid Friction.

(Including two varieties, both involved in train-resistance.)

(*a*) *Semi-fluid Friction*:—
  (1) Friction of ties, ballast, embankment, earthwork, etc.

(*b*) *True Fluid Friction*:—
  (2) Air friction at head of train.
  (3) ,, ,, rear ,,
  (4) ,, ,, sides ,,
  (5) Wind-friction.

though they were probably small compared with journal-friction, was truer in the first part than in the second, however; for all depended on the conditions. The Author's reference to journal-friction would lead to the inference that it was practically independent of the bearing-pressure and of the rubbing-velocity. The statement that the coefficient remained nearly constant with varying bearing-pressure was in apparent contradiction to experience. The difference in the train-resistance per ton, for trains of loaded and of empty cars running at the same speed, was first noted more than 30 years ago by Mr. A. M. Wellington, and had been noted by all who had made determinations of the train-resistance of loaded and empty cars since that time. Mr. Aspinall had found [1] that the train-resistance per ton was nearly three times as much for empty as for loaded wagons, under conditions otherwise comparable. Mr.

[1] Minutes of Proceedings Inst. C.E., vol. cxlvii, p. 199.

Mr. Mailloux.

A. C. Dennis in America had found[1] that, for American goods-wagons, the train-resistance per ton was 80 to 90 per cent. greater for empty than for loaded cars, run at the same speed and under comparable conditions. There was abundant evidence, of equally unquestionable character, that the train-resistance per ton was influenced by the load. It seemed difficult to account for this difference without concluding that the coefficient of friction did vary (inversely) with the bearing-pressure in some such way as it had been found to vary in the laboratory experiments of Messrs. C. J. H. Woodbury, A. M. Wellington, R. H. Thurston, and many others. The Author's conclusion that journal-friction was independent of speed was also in apparent contradiction to practical experience. It had been established, as the result of repeated tests and observations of all kinds, that the train-resistance had a relatively high value, sometimes termed "starting-resistance," ranging between 15 lbs. and 25 lbs. per ton at velocities near zero, and that this value decreased rapidly as the velocity increased, passing through a minimum of, usually, less than 6 lbs. per ton, at a certain critical velocity, which, according to Mr. Wellington, might be as low as 6 miles, and as high as 15 miles per hour, and which, according to the curves published by Mr. Dennis, might occur between speeds of 15 miles and 20 miles per hour. The figures given by the Author, being for speeds above 18·7 miles per hour, might be above the critical speed at which the minimum value occurred. The data cited were, in any case, too meagre to form the basis of any far-reaching or general conclusion, especially when they were flatly contradicted by the results of Mr. Wellington, Professor Thurston, and many others. The Author's conclusion did not seem to Mr. Mailloux to be warranted, especially as it apparently ignored the starting-resistance altogether, or made of it a separate difficult problem. What was needed was a theory of journal-friction which accounted satisfactorily for all the phenomena occurring from start to finish in a car-journal. The theory which he had found most useful, rational, and in harmony with the facts, was one which included the variation of the coefficient of friction with the speed in such a manner that a minimum value must and did occur at a critical speed. According to this theory, lubricated journal-friction might be regarded as made up partly of pure sliding (unlubricated) friction and partly of pure fluid-friction. The characteristics of both of the component frictions had been separately determined and were well known. The characteristics of pure friction

[1] Transactions of the American Society of Civil Engineers, vol. 1, p. 3.

Mr. Mailloux. between metal surfaces (unlubricated) had been determined by the aid of data obtained in 1878 and 1879, by the late Sir Douglas Galton.[1] Mr. R. A. Parke, one of the highest authorities on train-braking, had found, as the result of comprehensive study of various brake-friction data, more especially those given in the Papers of Sir Douglas Galton, that the relation between the coefficient of friction and the rubbing-velocity might be represented by curves of hyperbolic type (*Fig. 15*), for which he gave two empirical formulas.[2] In the case of fluids the friction was known to increase as some power higher than the first power of the rubbing-velocity. Therefore, the curve representing the relation between the coefficient of friction and the rubbing-velocity was of parabolic type. If, then, lubricated journal-friction was a composite phenomenon including both of these kinds of friction, the curve showing the values of the coefficient of friction from zero velocity to limiting velocities would be a curve such as could be made by taking, for each ordinate, the sum of the ordinates of a hyperbola and of a parabola. This meant that the curve would be a two-branch curve, having a minimum point at some critical velocity at which the total friction was made up of equal amounts of both kinds of friction. For velocities below the critical velocity the fluid-friction effect decreased, and the sliding-friction effect predominated. At velocities near zero the fluid-friction effect was negligible and the sliding-friction effect was a maximum. For velocities above the critical velocity the reverse took place: and at high velocities, the sliding-friction, while still existing, was small in comparison with the fluid-friction. Professor Thurston had given[3] data of careful determinations of the coefficient of friction for a wide range of rubbing-velocities (from less than 50 feet to as high as 1,200 feet per minute). The curves obtained by plotting these data (*Figs. 16*) indicated clearly the composite character of journal-friction. These curves showed the transition between "starting" friction and "running" friction—a change from a condition where the friction decreased to a condition where it increased with the velocity. As the different curves indicated, the critical velocity varied somewhat

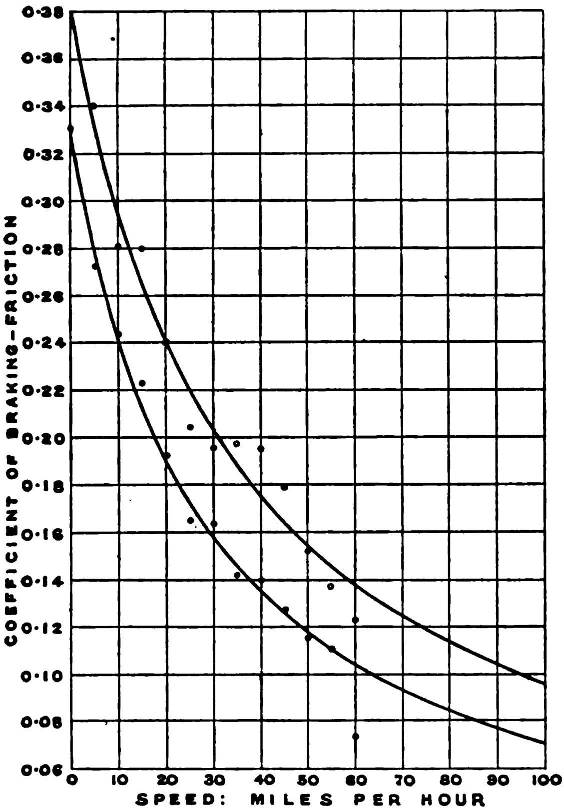
[1] "On the Effect of Brakes upon Railway Trains." Proceedings of the Institution of Mechanical Engineers, 1878, pp. 467 and 590; 1879, p. 170.

[2] R. A. Parke, "Railroad Car-Braking," Transactions of the American Institute of Electrical Engineers, vol. xx (1904), p. 235. See also "The Friction of Brake-Shoes," *Railroad Gazette*, vol. xxxiii, 1901, p. 405.

[3] "A Treatise on Friction and Lost Work in Machinery and Millwork," New York.

with the pressure and the temperature, and the effects of variations of temperature and pressure were not the same at low and at high rubbing-velocities. This was exactly what might be expected, if lubricated sliding-friction was a composite phenomenon, with components which preponderated at different rubbing-velocities. The portions of the curves which were beyond the critical velocity were Mr. Mailloux.

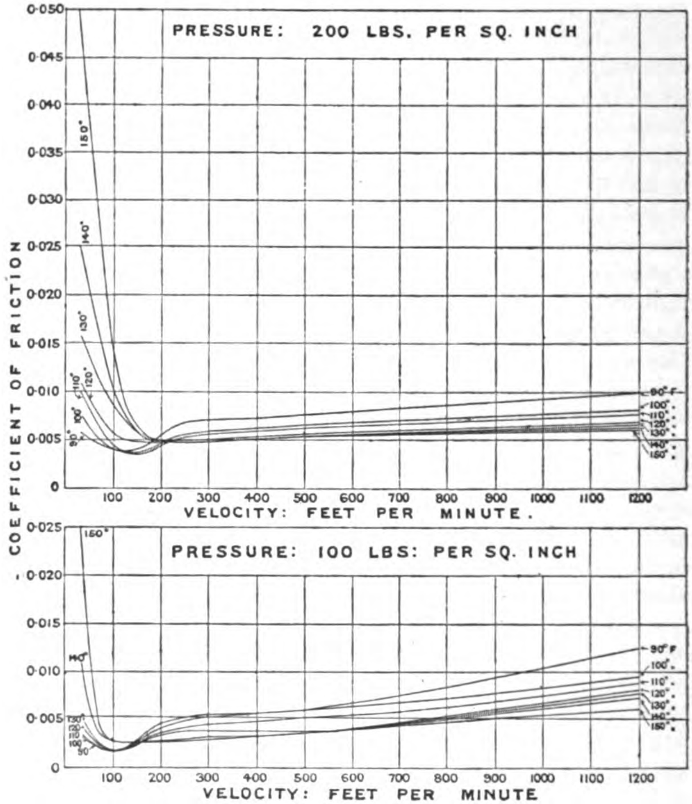
Fig. 15.



more nearly linear, and they represented values of the coefficient of friction which were more nearly constant for high than for low temperatures. Using Professor Thurston's values, Mr. Mailloux had been able to show that the discrepancies in the critical speeds at which the train-resistance was a minimum, as given by Wellington,

Mr. Mailloux. Dennis, and others, were due mostly to differences in the diameters of journals and wheels. It was largely a question of determining what train-speed corresponded with a given velocity of rubbing at the journal. There was no disagreement here between theory and practice. Journal-friction was always complicated by a certain amount of friction due to end-play of the journals in the bearings.

Figs. 16.



This friction, which was of slight importance when the brasses and the lubrication were both very good, assumed some importance when the brasses were rough, when the lubrication was inadequate, and when the road was not in first-class condition in regard to alignment and level of the rails at all points. These bad conditions of road and rail were favourable also to increase in wheel-flange

friction, and to increase in what the Author called track-resistance. Mr. Mailloux. It became very difficult, therefore, under such conditions, to segregate these different elements of train-resistance. The assumption made by the Author, that the journal-friction was constant under all conditions, might facilitate the process of obtaining certain component curves or graphs, which were supposed to represent the constituent elements of train-resistance in a given case. It did not by any means make these curves true or even plausible. On the contrary, in view of the preceding observations concerning the relation between the coefficient of friction and the pressure and rubbing-velocity, it left room for doubt whether any of the component curves were correct. This assertion became all the more significant when it was considered that the two elements which the Author called rolling-friction and track-resistance, being merely phases of the same thing—rolling-resistance—were still more difficult, if not impossible, to segregate. There was much misapprehension and misunderstanding about "rolling-friction." The term itself was a misnomer, and was misleading, being an inheritance from the last century, like the so-called "coefficient of rolling-friction" still mentioned in text-books, although it was not a coefficient at all, since it represented a distance instead of a ratio-factor. These misconceptions of rolling-friction were seemingly the result of an incomplete or imperfect analysis of the energy-reactions involved, and of a consequent failure to note the precise manner in which power was expended and energy was absorbed in doing the work incidental to this kind of "friction." It could be shown that, in every case, the power expended in causing a wheel to roll over any surface was expended in substantially the same manner as if the wheel were ascending a certain equivalent gradient. It made no difference whether the surfaces in rolling contact yielded or not. When they yielded there might be crushing and mangling effects. When they did not yield, there might be shocks. These were merely incidental to the dissipation of the energy abstracted from the moving source which caused the rolling action. When the surfaces in contact were both very smooth, the resistance to rolling, and the force required to overcome it, were both very small. The slightest irregularity in the surfaces in contact obstructed the rolling motion and increased the tractive force required to produce it. Any elevation, however slight, in either surface, would require the rolling object to be lifted bodily in passing over it. Any depression would cause the rolling body to sink to a lower level, from which it must be subsequently lifted, just as it was in passing over an elevation. Hence, every

Mr. Mailloux. irregularity in the surfaces which came in rolling contact, occasioned some lifting action of the rolling object, against its weight and the weight resting upon it. The energy consumed depended, obviously, upon the total weight lifted, the height to which it was lifted, and the number of lifts per unit of time or distance. In a railway-vehicle, every irregularity in the wheel-tread involved a lifting action at each revolution of the wheel, and every irregularity of level in the track involved a lifting action as each wheel passed over it. The hollow caused in the track by the yielding and bending of the rail under each wheel also produced a slight but continuous virtual gradient under the wheel. A sanded or muddy track involved a series of such lifting actions which might follow each other so closely as to constitute a continuous lifting action. The complete analysis of the phenomena[1] showed that, in all cases, rolling-friction might be regarded as something which in its effect was equivalent to a gradient. This view of rolling-friction—more properly rolling-resistance—gave the clearest idea of the manner in which it consumed power, and, at the same time, furnished the most practical way of estimating its amount, as an element of train-resistance, under different conditions. He had found it convenient to consider separately the rolling-resistance due, (*a*) to mangling or crushing effects; (*b*) to non-yielding inequalities of surface; (*c*) to yielding of the track, or "track-hysteresis." The mangling or crushing effects included the increased train-resistance due to sand, mud, ice, snow, etc., which was of special importance in the case of traction over city streets, especially when grooved rails were used. The possibility of accounting for the increase in train-resistance due to these obstructions had proved very useful. Incidentally, it enlarged the scope of many formulas for train-resistance. The equivalent gradient representing the effect of these obstructions might vary from 0·1 to 1·0 per cent. (1 in 1,000 to 1 in 100) or more, corresponding with 2 to 20 lbs. or more per ton (of 2,000 lbs.). Under average street-railway conditions, the journal-friction was always less than the rolling-resistance, instead of more, as stated by the Author. It was seldom less than 5 lbs., commonly 8 to 10 lbs., and at times as high as 15 lbs., per ton. On steam roads, or on electric roads not over public streets or highways, when tee-rails could be used, the track could be kept relatively clear and clean. In the case of deep and drifting snows, however, the rail was often covered again by the snow, immediately after each wheel had passed. In such cases, although the snow yielded readily under the

[1] See *Harvard Engineering Journal*, vol. iii, p. 269, and vol. v, p. 39.

Mr. Mailloux.

pressure of each wheel, and therefore the resistance was very slight, yet, being present at each wheel of the train, and its amount being practically the same at all wheels, it might sometimes cause a material increase in the train-resistance per ton. The non-yielding inequalities were exemplified by open or imperfect rail-joints, the gaps at switch-frogs, and the high and low spots in the rails, due to wear or to original defects or to imperfect laying. These were all obstructions of the intermittent kind. In the case of long trains running at considerable speeds the effects of the obstructions coalesced, the result being virtually the same as if they were replaced by a continuous obstruction, the value of which, as the preceding considerations showed, would obviously increase with the speed of the train. This conclusion was not in harmony with that of the Author, who considered both rolling-friction and track-resistance to be independent of the speed. There was also a certain amount of rolling-resistance due to the wheel-flanges, which also increased with the speed and with the inequalities of the track-alignment. Mr. Mailloux had adopted the term "track-hysteresis," first suggested by Mr. A. Mallock, in the discussion on Mr. Aspinall's Paper, and had used it, with a slightly extended meaning, to designate generally all yielding-effects due to continuous obstructions which were elastic in any degree and which did not involve permanent deformation. In all such cases the (theoretically) recoverable energy stored in the track by compression was not, practically, all recovered. It might be assumed that some of the parts, such as the sleepers, ballast, ground, etc., were compressed somewhat beyond the elastic limit at the expense of additional force over and above what would be required if the limit of elasticity were not exceeded at any point. The energy corresponding with this additional force was non-recoverable, being expended in some form of molecular friction and ultimately converted into heat. A certain amount of set was at the same time produced in the parts compressed beyond the elastic limit. The restoration of these parts to their original condition, after the wheel had passed, involved a second expenditure of energy, substantially equal to the first, and coming obviously from the recoverable energy stored in the parts which had not been compressed beyond the elastic limit. Thus each time that the track was depressed momentarily there occurred a cycle of reactions which caused the abstraction of energy from the moving body and its dissipation by friction, etc., in much the same way as in magnetic hysteresis. While there was a theoretical distinction between the train-resistance due to non-yielding inequalities, and that due to track-hysteresis, it was very difficult, if not

Mr. Mailloux. impossible, in the present state of knowledge and facilities for measurement, to separate them quantitatively in practice. For this reason, Mr. Mailloux did not see the logic of separating rolling-friction from track-resistance, as the Author had done in his classification. In connection with the Author's reference to air-resistance Mr. Mailloux was amused to find him seriously proposing to estimate the force on the front and rear of a flat plane by the equation $P = 0 \cdot 00254\ \omega\ V^2$, without any correcting factor. In the discussion of Mr. Aspinall's Paper, this same formula had been put forward by several members, and had been accepted, apparently by all, with the exception of Mr. Aspinall himself, who, in the summing up, had gone a step farther in the wrong direction by proposing to increase the constant to 0·0054, his demonstration and formula being substantially those first given by Newton. Two Italian scientists, Drs. G. Finzi and N. Soldati, by a series of brilliant experiments[1] in which the pressure against bodies moving in air was measured manometrically at every point of the surface, had furnished undeniable proof that the theoretical formula above referred to, which was due originally to Euler, gave the correct pressure for one point only, namely, at the centre, in front of the plane. At all other points the actual value was less than this theoretical value. There was, however, a suction effect at the rear of the plane which was ignored entirely by the formula, and which compensated in part for the excess of front-pressure given by it. In certain cases the theoretical formula could, according to Drs. Finzi and Soldati, be used for planes, with a correcting factor which varied between 0·73 and 0·75. For planes which were not at right angles to the line of motion, the theoretical formula required, of course, still further modification by the introduction of trigonometrical coefficients. For bodies of irregular form, the correcting factor became still more complicated. The perusal of the Paper of these scientists left the impression that the theoretical formula should, in general, be taken with more than a grain of salt, in the form of correcting factors. There were two facts, of importance in connection with the estimation of the effects of air-resistance on moving trains, which ought to be known and appreciated more than they were. The first was the discovery—made in the last century by the French investigator Dubuat, and known in French technical literature as "Dubuat's paradox"—that the pressure against a plane was always less when the plane was moved in a perfectly still fluid than

[1] "Esperimenti sulla dinamica dei fluidi." Milan, 1903. (Lecture to the College of Engineers and Architects in Milan.)

when the fluid was moved against the plane at the same mean velocity. It had been shown by Professor A. Rateau in an article on the Pitot tube,[1] that this phenomenon was due, primarily, to the circumstance that in a current of air the velocity was never uniform for all parts (elements) of the air-current. Since the pressure produced was proportional to the square of the velocity, the total result was proportional, as Professor Rateau had shown mathematically, not to the mean velocity but to the mean of the sum of the squares of the velocity. Professor Rateau had also proved this by ingenious and conclusive experiments, which incidentally explained many of the anomalous and discrepant results obtained when the velocities of fluids were measured by the Pitot tube. He had also explained fully, in the article in question, the conditions and corrections which were essential to render the Pitot tube a reliable instrument for the measurement of the velocities of fluids. This had also been done by Professor F. E. Nipher in America, and by Messrs. Finzi and Soldati in Italy. The second fact above referred to, discovered by von Lössl several years ago, was that when a body, as it moved forward, also oscillated from side to side, the effect was the same as if its effective area were increased. Both of these facts helped out the theoretical formula a little, by making the correcting factor come a little nearer to unity; but they did not, in the majority of cases, make the formula true. It always needed a correcting factor. Mr. Mailloux was of the opinion that no experiments bearing upon the determination of the air-resistance to trains of any length and form had yet been made which had given more practical and valuable information than those of Professor W. F. M. Goss, at Purdue University, made with small models of railway-coaches placed in a tunnel through which a current of air was passed. These experiments[2] had given for the first time, and apparently with a fair degree of precision, the distribution of the total air-resistance at different portions of a train. It was, true that, previously, the experiments of von Lössl, in Germany, with lighted tapers placed in front of moving bodies, had revealed the existence of a mass of stationary air forming a "cone" in front of the moving body. But von Lössl's ideas regarding the distribution of the resisting force of the air were, as was now known, entirely erroneous. He was led, as so many had been, by the seeming plausibility of the hypothesis of Euler, to place too much Mr. Mailloux.

[1] "Expériences et théories sur le tube de Pitot et sur le moulinet de Woltmann (hydromètres et anémomètres)." *Annales des Mines*, vol. xiii (1898), p. 331.

[2] *The Engineer*, vol. lxxxvi (1898), p. 164.

Mr. Mailloux. reliance on the theoretical formula. His confidence in it was such as to lead him even to ridicule the suggestion that there might be a force due to back pressure or suction, at the rear of a moving body. The quantitative (dynamometric) results furnished by Professor Goss' experiments had not only settled this point, but they had also given certain ratios which were definite and seemed to be very nearly if not quite constant in all comparable cases. They had shown conclusively that the larger portion of the total air-resistance occurred at the front and rear ends of a train; that the air-resistance was virtually constant for all intermediate coaches of the train, that of each being approximately equal to one-tenth of the air-resistance at the head of the train, and a little less than this at the second coach; and that the ratio of the air-resistances at the front and rear ends was substantially constant, independently of the length of train, the air-pressure at the front end being approximately four times greater than that at the rear end, for coaches of ordinary shapes. Mr. Mailloux had found that train-resistance formulas based upon the results of these experiments had, in practice, given results which compared very favourably in precision with those obtained by more pretentious formulas. It was interesting and significant that the formulas of Mr. W. J. Davis, Jun., used by the Railway Department of the General Electric Company, in America, approached very closely, in their amended form (with smaller constants) the formulas based upon the Goss experiments. Up to the present time the investigations of Messrs. Finzi and Soldati and of Professor Goss were in many respects the most valuable of all contributions to knowledge of the subject of air-resistance. It was obvious from the preceding comments on the theoretical formula used by the Author, that all his calculated values for air-resistance were likely to be inaccurate, being probably too high in most cases. His values for journal-friction were obviously too low, in most cases, at the higher speeds. Consequently, the curves representing the other elements of train-resistance were likely to be wrong, because they represented values which were too small. In conclusion, a word of caution might be said about resistance-equations. The Maclaurin power-series $y = A + Bx + Cx^2$ had always been popular with designers of new train-resistance formulas, and, to do it justice, it had done good service. It was undesirable, however, to dignify this equation with the term general equation until it could be completed by the introduction of a term which would provide for starting-resistance. The complete general equation for train-resistance would be one which gave, at zero velocity not the value 0, but a value equal to the starting (static) resistance.

Mr. Mailloux. In order to do this, there must be introduced a hyperbolic term, or a term in which the velocity appeared with a negative exponent, thus

$$R = A + Bx + Cx^2 + Dx^{-n}.$$

Mr. Mailloux had also found that the train-resistance curve could, in some cases, be expressed symbolically by an equation of the form

$$R = A + \frac{B}{(x + a)^n} + Cx^m,$$

where m was a non-integral exponent, whose value ranged between 1·6, and 2, while n had a value ranging between 1 and 2, and a was a constant. The objection to such formulas was that it was more difficult to determine and to use a non-integral exponent than an integral exponent. Moreover, this exponent might change for the same formula, when the conditions (length and weight of train, etc.) changed. The Maclaurin type of formula was, for this reason, preferable. In using such formulas, he added a virtual-gradient term G, to which different values were assigned according to the quality and the condition of the track.[1] It was obvious that G could be made to include the virtual gradient due to snow, mud, or in general, to any cause affecting the rolling-resistance. In this way, the scope of any train-resistance formula might be greatly increased.

Mr. Mallock. Mr. A. MALLOCK observed that the Author represented the resistance by an expression of the form $A + Bx + Cx^2$. Any curve (with the proper limitations as to the character of the differential coefficients) could be approximated to by such a formula; but the methods by which relative values of the coefficients were obtained was open to objection. He would merely refer to the notes in the Appendix which contained the Author's theory. Note 1 took no account of the resilience of the track and assumed that the whole work done in depressing the track was lost. What was really lost was only the difference between the work done on the track during compression and that given back by the track during expansion. If a weight were rolled over a perfectly resilient track, no work was required, whatever might be the depression caused by the load. Note 2 was unsatisfactory in several ways. He would merely point out that the Author took the flange-action he described to be continuous, but observation showed that where the line was straight the flanges came into play alternately on either side and for considerable intervals of time were not engaged at all; hence it was not

[1] See W. C. Gottshall, "Notes on Railway Economics and Preliminary Engineering," 2nd ed., p. 157. New York, 1904.

Mr. Mallock. quite correct to take the mean of the extra tractive force due to flange-friction as equal to its maximum value. Further, the inertia of the mass of the carriage could only increase the lateral pressure between the flange and the rail for a small fraction of the time during which the plane of the wheels was changing in azimuth. What chiefly determined and limited the lateral force on the flange was the resistance of the tread of the wheel to lateral slip. The Author did not notice a source of resistance which generally, Mr. Mallock thought, outweighed the flange-effect, namely, the slipping which must occur between wheel and rail due to the difference of the effective diameters of each pair of wheels when the flanges were not equidistant from the rails. This difference depended of course on the degree to which the tires were coned.

Mr. Sayers. Mr. Henry M. Sayers remarked that the principal novelty in the Paper was the Author's analysis of train-resistance into the three parts described, and in particular his attempt to identify that part of the resistance which varied as the first power of the velocity with "flange-action," measured by certain proportions and the mass of the truck or coach. Referring to Note 2 of the Appendix, the first assumption there made was that the truck ran with an average or mean inclination to the rail depending upon the length of the wheel-base and the play or clearance between the flanges and the rails. This meant that the truck was constantly swinging about the pivotal point, and that some two flanges were bearing against the rails throughout the whole of a run on straight track. It also meant that, other things being equal, the resistance due to this action was proportional to the play or clearance. These propositions would not be readily accepted. First, the coning of wheel-treads, and the damping effect of bolster- or pivot-friction, assisted trucks to run for considerable distances on straight tracks without the flanges touching the rails at all. The good running-qualities of different trucks were largely dependent upon the suppression of swinging, especially when the trucks were heavily loaded with motors, and freedom from swinging-propensities should be aimed at in their design. Secondly, while long wheel-bases and small flange-play were no doubt conducive to minimum resistance on straight tracks, this was not so on curves. There the wheel-base and play should be such that no flange-grinding occurred, except that inseparable from the guiding action of the rail upon the truck. The radius of the sharpest curve traversed was therefore the factor which should regulate the ratio of play to wheel-base, and probably this accounted for some of the differences noted in the practice on various railways. Rolling stock must be designed to suit the more

difficult rather than the easier circumstances; so that even if the Author's principle were true for straight lines, the increase of wheel-base and diminution of play, while diminishing train-resistance on straight track, might increase it seriously on curves and prove to be, on the whole, a disadvantage. Mr. Sayers.

Mr. W. N. Smith, of New York, wished that the time at his disposal had been sufficient to examine more experimental data than was possible under the circumstances. The Author's method was so carefully worked out that it would be desirable to compare by means of it all the experimental results obtainable. Train-resistance in general might be regarded in two aspects: first, the general degree in which it entered into railway-working, and secondly, the computation of resistance in detail as shown in the Paper. In the work of predetermining the motive power of electric trains, it was necessary to take account as closely as possible of the actual conditions imposed upon the operation of a given train or trains. The most important things affected by the train-resistance were the capacity of the propelling motors and the speed of the train. On a relatively small system, involving comparatively few motor-coaches for interurban trolley-service, as it was commonly called in the United States, the question of train-resistance was of relatively small moment, and any one of several formulas that had been fairly well verified by practice would answer every purpose in all the predetermination work that was called for by such a problem. Work of this sort constituted the large majority of electric-railway problems commonly met with. But when it became necessary to make careful estimates for very large railway-electrification problems, the amount of equipment involved was so large, and the congestion of traffic and the short headway between trains required such close estimates of speed, that considerable attention must be given to this particular factor. Its importance was greater where the conditions required a relatively large amount of coasting from maximum speed to the point of brake-application, and it was in this connection that a close study of train-resistance might enter into the predetermination of the motor-capacity, especially when it became necessary to estimate very closely the performance that could be expected of some particular motor, which, for reasons of space occupied, or other motives of economy, it was desired to use. Here again, however, it had been usual to employ one of several resistance-formulas that had been fairly well borne out by experiment. Predetermination of the power and equipment for a railway-system was based upon so large a number of variables, many of which had to be assumed in order to arrive at a result, that Mr. Smith.

Mr. Smith. the error introduced by a slightly incorrect assumption of train resistance eventually became a relatively much smaller source o error in the entire problem. To that extent the exact determina tion of train-resistance could not justly be regarded as of greate importance than the determination of numerous other factors any of which were likely to be slightly inaccurate in preliminar estimates. The method proposed by the Author was the bes instance known to Mr. Smith in which the conditions enterin into the total result had been carefully taken into separate account and given their due proportion in making up the total result It had usually been considered that some one formula could be pro duced, fitting practically all conditions, although the failure of most of the previous attempts in this direction was generally recognized The Author's method should therefore merit careful consideration from railway-engineers, for he had brought it down to a very reasonable and practical basis. The only check Mr. Smith had been able to make, where all the conditions were known that could be compared in the manner outlined by the Author, was in the case of a test record of a single motor-coach, and this showed that the Author's method agreed fairly closely with actual practice; but neither in this case, nor in two others where such checking was attempted, had the resistance calculated by the Author's method been as high as the actual resistance, nor as close as when calculated by either the Sprague formula, or that proposed by Mr. Smith.[1] The constants calculated by the Author's method did not seem quite large enough for motor-coaches; though checking of still other results might modify this conclusion. Further investigation should be made of the resistances of trucks carrying electric motors, and particularly of the effect of the gear- and pinion-friction in the motor. When a train was coasting the gear- and pinion-friction constituted a resistance that had to be estimated separately. Though usually included in the motor characteristic curves which gave the net tractive effort of the motor (after transmission through gears and pinions), while current was being applied during acceleration and full-speed running, it entered in as an extra resistance after the current was cut off and the train was coasting, and must then be added to the other elements of train-resistance. It seemed exceedingly difficult to estimate at its proper value the flange-wear of the wheels. On a new equipment

[1] O. S. Lyford, jun., and W. N. Smith, "Problems of Heavy Electric Traction," Transactions of the American Institute of Electrical Engineers, vol. xxiii (1905), p. 691.

the flange-wear was practically nothing, but within a few years Mr. Smith. flanges in heavy service frequently wore sharp, sometimes only on one side of the truck, and the wheel-treads assumed noticeably different shapes, which could not but affect the flange-action of the trucks as computed by the Author. It would be interesting to know whether any attempt had been made to compare the flange-action of a new truck with that of an old one in which the treads and flanges have been subjected to considerable wear. The effect of air-resistance on the sides and roofs of the coaches was further complicated by the presence of eddies in the air at the spaces between the coaches. Anyone who had ever walked from one coach to another of an American train in motion knew that there was a violent circulation of air across the platforms at the ends of the cars, and whether this effect might or might not be susceptible of mathematical treatment, it seemed to Mr. Smith that some information about it might be ascertained experimentally. Further tests along the lines followed by the St. Louis Test Commission could profitably be made, with the test-car placed in the middle of a train, as well as at either the front or the rear. It was quite possible that the air-resistance of the second, third, or fourth car in a train of motor-coaches or of other cars might be a different quantity in each case, and the effect of the position of a car in a train was worthy of further study. The large railway-problems of the future involving close estimates of train-resistance were in all probability those wherein electric motive power would be called into play, and for that reason all experimental information bearing upon the subject of electric motor-coach propulsion should be analysed in just such a careful and scientific manner as had been done by the Author, with data from various kinds of coaches, trucks, and types of motive power. In conclusion, he congratulated The Institution and the Author upon the clear, simple, and scientific treatment of this complex subject.

Mr. H. B. TAYLOR observed that the Paper was of very Mr. Taylor. practical benefit to railway men, and would help towards more efficient working of railways by pointedly illustrating the relative values of the different elements of train-resistance. For instance, the fact that flange-resistance varied as the side play showed the importance of keeping this play small; but Mr. Taylor had met engineers who insisted on the track being laid slack to gauge on the straight. He had known permanent-way inspectors in India who advocated keeping one rail of the track slightly lower than the other, to ensure true steady running. This would be achieved; but if the cant were considerable, side friction would occur from the flange pressing against the lower rail. He would like to hear the

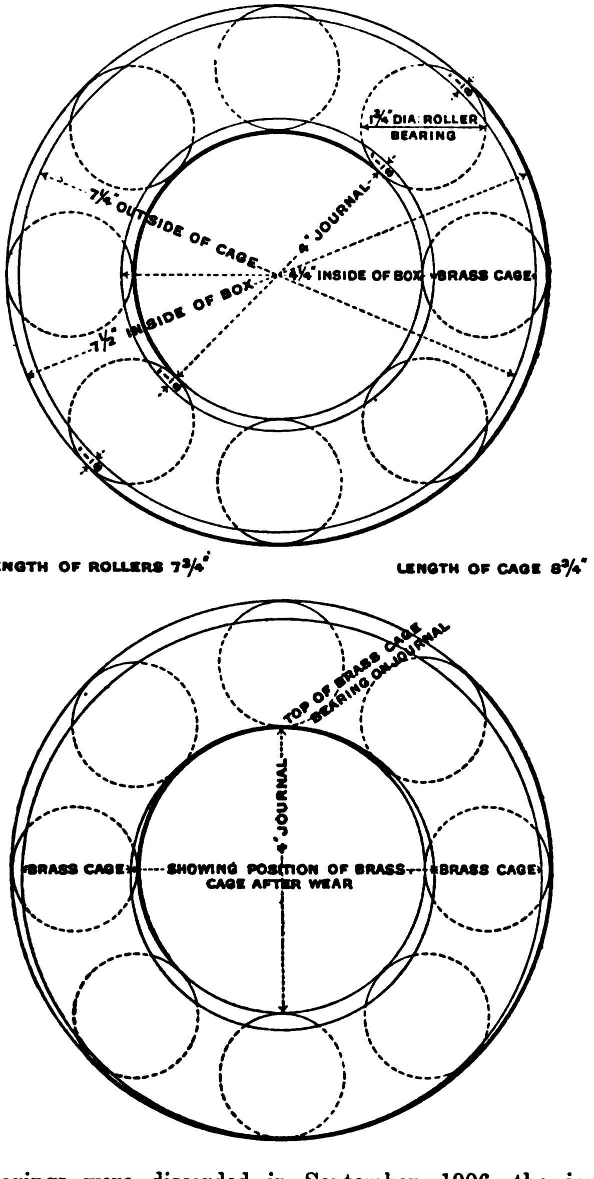
Mr. Taylor. Author's views on this point. It would be interesting to know what was the side play on the Brompton and Piccadilly tube stock compared with that on the stock on the District Railway. In the tube the running was very smooth, while on the District line, standing at the end of a coach, it was difficult at times to keep one's feet. He would like the Author to go farther and deal with curve-resistance in a similar manner. On curves there arose, in addition to others, a new resistance, buffer-friction, which was very considerable with long bogie-coaches on sharp S curves. It was within his own experience, when dealing with construction-trains on the temporary Bolan Railway, that, though on the straight a bogie-truck was reckoned as load equal to two four-wheeled vehicles, on sharp curves the train-load was in favour of four-wheeled vehicles. These curves were usually coupled with the limiting gradient of 1 in 20, and a long coach on such a track no doubt got a twist which added to the train-resistance. He drew attention to this, as bogie-trucks were usually considered advantageous on a railway with sharp curves; but the advantage would be lost on hill-railways if the length of the stock were unduly increased. The Author pointed to the advantage of roller-bearings in the running of goods-stock at low speeds. A point to note in practice was that, through careless shunting and other accidents in yards, breakage of axle-boxes was continually occurring. Such special axle-boxes cost, he believed, £5 more than an ordinary box.

Mr. Williams. Mr. J. P. Williams, Officiating Locomotive Superintendent of the Eastern Bengal State Railway, forwarded through Mr. W. R. Haughton, the Engineer-in-Chief of that line, eight sun-prints showing how roller-bearings had worn and rolled the journals.[1] In January, 1899, two first- and second-class composite coaches (with wooden underframes and non-vacuum brakes) were fitted with roller-bearings, and each ran about 165,000 miles in $4\frac{1}{2}$ years. The journals had remained parallel and showed very little wear, but the cages had come down as shown in *Figs. 17*, which also showed the same bearing when new. These cages were replaced by others in which the outside diameter was reduced to $7\frac{1}{16}$ inches and the inside diameter increased to $4\frac{3}{8}$ inches, thus increasing the clearance between the cage and the journal. These

[1] The journals in question were all originally 4 inches in diameter and about 9 inches long. Many of them have worn taper, the reduction of diameter at the smaller end—which in some cases is the inner and in others the outer end of the journals—varies from $\frac{1}{32}$ inch to 1 inch. Each journal was fitted with eight rollers (except in one case where twelve rollers were fitted) and the prints show the results of $7\frac{1}{2}$ months' running under bogie-coaches.—Sec. Inst. C.E.

Mr. Williams.

Figs. 17.

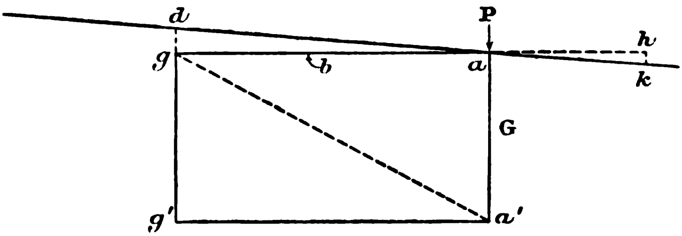


roller-bearings were discarded in September, 1906, the journals having worn taper, and brass bearings were substituted.

Mr. Wolley-Dod.

Mr. F. Wolley-Dod thought that there was an error in the method adopted by the Author in computing the resistance due to "flange-action," the item Bv in equation 7 on p. 233. On p. 230 it was stated that vehicles continually moved at a slight angle to the rail. It would be more accurate to say that they continually oscillated. During a part of each oscillation the wheels ran free, but diagonally to the rails; during the second part, the leading wheel ran up against the rail, and a horizontal reaction, such as P in *Fig. 12,* on p. 264, was generated, which rotated the vehicle or bogie about a vertical axis, and sent it off again towards the other rail; the third and fourth parts of each oscillation brought a repetition in the reverse direction of the first and second. The magnitude of the reaction P was determined not so much by the inertia of the vehicle or bogie about the axis round which it was rotated, as by the moment about that axis of the frictional resistance of the wheels on the rails. Moreover, the "train-resistance" had little to do with the magnitude of P, as there was very little rubbing between wheel and rail at the point at which the reaction acted, and the work absorbed was of the same nature as rolling-resistance, which was stated on p. 230 to be a negligible quantity. The chief item in the resistance caused by this oscillation was the work done in sliding the wheels during each rotation. The amount of this sliding might be ascertained thus. In *Fig. 18*, which was *Fig. 12* of the Appendix, completed to show the other two wheels, $a\ g = a'\ g'$ = the wheel-base b, and $a\,a' = g\,g'$ = the distance between centre of rails G. If the weight at each of the four points a, a', g, and g' were W/4 and μ were the coefficient of friction, the force necessary to make any one of the wheels slide on the rail was μ W/4. A horizontal force such as P would cause each wheel to slide about g or g' as centre, provided the wheel-base b was more than about $1\frac{1}{4}$ times the distance between the centres of the rails, G. Taking moments about this point

Fig. 18.



$$P \times b = \frac{\mu\,W}{4}\left(b + G + \sqrt{b^2 + G^2}\right).$$

Mr. Wolley-Dod.

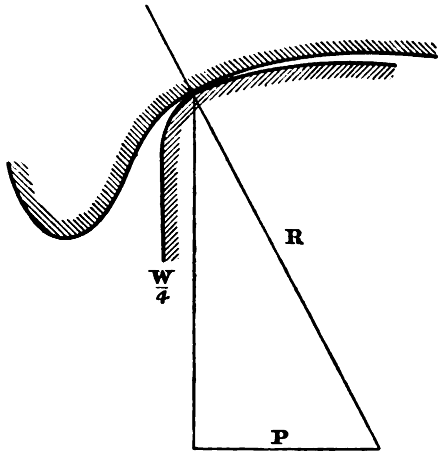
(If b was less than about $1\frac{1}{4}$ times the gauge, the sliding would take place about a vertical axis situated about $1 \cdot 5$ G behind the leading axle, and the numerical value of P would be greater, as all four wheels would slide). Taking the Lancashire and Yorkshire bogie-vehicles, in which $b = 6 \cdot 5$ feet and $G =$ about $4 \cdot 9$ feet, $P = 0 \cdot 75 \mu W$, W being the total weight on all four wheels of the bogie. This value of P applied only to those particular dimensions; if the wheel-base was increased, P was decreased, and vice versa. But for any possible proportions, velocity, and coefficient of friction, the value of P was considerably greater than that required to overcome inertia by the Author's formula on p. 264. During each complete oscillation the vehicle or bogie was deflected, first through twice the angle $d\,a\,g$; it then ran across against the other rail, when it was deflected back again through the same angle, and the work done was equal to the sum of the distances which each wheel slid during the two deflections multiplied by μ W/4, which if all wheels ran on the same diameter was

$$\frac{4 \times \text{length of arc } d\,g}{\text{wheel-base } a\,g} \times \frac{\mu\,W}{4}\,(b + G + \sqrt{b^2 + G^2}).$$

Taking $d\,g$ as $\frac{1}{16}$ foot, which it was stated to be in the Lancashire and Yorkshire bogies, this was $0 \cdot 1875\,\mu W$ units of work expended while the vehicle traversed a distance corresponding with one complete oscillation, and the average resistance was this amount of work divided by this distance. Here arose the difficulty of ascertaining the distance, and on examining the details, it was found that both the distance and the possible degree of obliquity depended on a number of items, comparatively small variations in any one of which would affect the result appreciably. The most important were the clearance and the wheel-base; but even with a given wheel-base, and a nominally constant clearance, speed, and all other conditions, the upper line in *Fig. 9* (p. 259) showed considerable variation in both the amplitude and the period of each oscillation. Whatever radius were given originally to the fillet between the flange and the tread of a wheel, and to the shoulders of the rail, the result of wear was such that, when the flange approached the rail, the radius of the flange at the point of mutual contact was greater than that of the shoulder of the rail. The radius of the fillet in new wheels was nearly always made slightly larger than that of the rail-shoulder. The result was that, as the flange approached the rail, the fillet tended to ride on the shoulder of the rail, and to lift the tread clear of the rail-head.

Mr. Wolley-Dod. These conditions were shown in a very exaggerated form in *Fig. 19.* The reaction between wheel and rail took place along R, the vertical component of which was W/4, the weight on the wheel, and the horizontal component was P, the lateral force tending to rotate the vehicle or bogie about a vertical axis. The wheel continued to mount, and the inclination of R to increase, until the horizontal component P became large enough to produce rotation in opposition to the sum of the resistance of the wheels to sliding; and the axis about which this sliding took place was determined by the minimum value of P, the moment of which about that axis was equal to the moment of the resistances. But the wheel would continue to mount and P to increase after the

Fig. 19.



sliding began, because no force however great could change with absolute suddenness the direction in which the wheels were travelling. Further, in order to do work, P must be in excess of the resistance, and it must also overcome the inertia before it could accomplish the work; on the other hand, directly sliding commenced, the coefficient of friction, and consequently the resistance, was reduced. The fact that inertia had to be overcome was one of the chief reasons why oscillation once started would continue; it was also one reason why the moment of inertia of a vehicle or bogie about a vertical axis was a factor affecting the amplitude and violence of oscillation. The wheel which thus ran on its fillet traversed a greater distance in one revolution (or in a given fraction of a revolution)

than the other wheel on the same axle; this reduced the distance which the wheels had to slide, and gave rise to a rotation couple tending to reduce the other forces required to produce rotation. The trailing wheels of a vehicle or bogie were less liable to violent oscillation than the leading, and their movements did not necessarily synchronize with them; when the leading wheel was in the position shown, the trailing wheels might be out of centre either towards the opposite rail, or towards the same rail, or in an intermediate position, and this consideration affected the obliquity of the whole vehicle, and consequently the angle through which it was deflected at each oscillation. The speed of the train was unquestionably a factor which affected the violence of oscillation, and consequent resistance, and it was a well-ascertained fact that a given vehicle on a given track frequently ran more smoothly at a high speed than at a low one. An item of train-resistance not noticed by the Author, which was affected by the moment of inertia of a vehicle as compared with that of a bogie-truck, as well as by the speed of the train, was the work lost in producing local depression of the rails, and overcoming friction of springs, axle-boxes, etc., as a result of the pitching of the vehicles. The average resistance due to these was considerably less per gross ton for bogie-vehicles than for four-wheeled stock, but on good track the total resistance from this cause was probably a comparatively small fraction of the total train-resistance. As a consequence of assuming the Cv^2 item of the resistance—in which he included only air-resistance, though probably an appreciable amount of "miscellaneous resistance" varied as v^2—to have been the same per ton for the bogie-vehicles as for the four-wheeled vehicles on the Northern Railway of France, the Author had too large a value for the Bv items. This error was noticeable in *Fig. 4* of the Paper. If C were slightly increased and B slightly reduced, the thick line would correspond more closely with the circles in this diagram. Mr. Wolley-Dod.

The AUTHOR, in reply, observed that Mr. Armstrong was quite right in pointing out that the formula he gave was applicable only to the particular type of coaches tested. The Author had not the necessary data to enable him to work out the resistance of these trains by the method given in the Paper, but Mr. Armstrong could easily satisfy himself of its applicability to the trains in question. The Author had endeavoured to secure the data of the cars tested by Mr. Smith, but was unable to ascertain the most important figure, in his estimation, namely, the actual clearance between the rail and the flange of the wheels, as tested. Unfortunately, this was a dimension rarely noted, and the Author agreed with Mr. Smith in The Author.

The Author. his remarks as to the influence of the wear of the flanges on the resistance. He would be much interested to see some confirmation of Mr. Cardew's opinion that the oscillation of the bogies diminished as the length between bogie-pivots increased, and that it became of practically no importance in very long vehicles, as it was contrary to his experience, which went to show that there was nothing in lengthening the coach, *per se*, that tended to steady the bogie. He would of course admit that increased weight on the bogie-pivot helped to steady the bogie, as had been pointed out by Mr. Sayers, but the friction thus produced was quite as likely as not to hold the bogie permanently over in one direction, so that there was no real saving in flange-resistance. The oscillograms given in *Fig. 9* had been taken from a record of a continuous run of about 35 miles on an English main line at speeds ranging from 40 to 50 miles per hour; and they represented very fairly the oscillations of the bogies over the whole of the run, indicating that Mr. Cardew's view that such oscillations only took place under exceptional circumstances required some modification. The classification of the elements of train-resistance proposed by Mr. Mailloux was of interest from an academic standpoint, but it did not appear to add much to a clear comprehension of the relations of these elements from a practical point of view. It might be true that the friction of ties and ballast and the friction of the air on the train were both included under the expression "fluid friction," but it was difficult to see what practical result could be achieved from this grouping. In the same way flange-friction and journal-friction were no doubt both cases of sliding-friction, but beyond that they had nothing whatever to do with one another. Mr. Mailloux seemed to think that the form of equation $R = A + Bv + Cv^2$ adopted in the Paper was accountable for the classification used by the Author, whereas the reverse was the case: the Author had examined the most important elements of resistance and grouped them according to their relation to the speed, and he submitted that this was the only practical method of classification. He would point out, however, that the value of this method of classification lay in the help that it afforded in the interpretation of the results of tests as expressed by curves. Thus, for example, when it was known that the form of every resistance-curve was that given above, an inspection of the curves in *Fig. 2* showed that the difference in the resistance of four-wheeled and bogie coaches was due to that element which varied as the first power of the speed, that was, to flange-action, a result of the greatest practical importance. The Author deprecated the use of formulas in the manner suggested by Mr. Mailloux. The attempt

to represent by a formula the resistance of a train was justified only so long as it was recognized that the formula could refer to that train and no other, unless it were made up of rolling stock of identical character in all respects. There was of course no difficulty whatever in devising a formula that should represent the results of any given test or set of tests: as Mr. Mailloux had done. But such formulas threw no light upon the problem of the predetermination of train-resistance for trains of a different character. A glance at *Fig. 8* in the Paper was sufficient to show how hopeless was the attempt to obtain a formula that should represent even approximately the resistance of trains such as those there referred to. The Author had endeavoured to trace to their causes the different elements of train-resistance, and to determine from actual tests, not by calculation, in what way these elements depended upon the rolling stock employed, so as to make the results universally applicable. With regard to Mr. Mailloux's objection to the view that journal-friction was independent of the load, the Author had shown that with perfect lubrication the coefficient of friction varied inversely as the pressure, but that with lubrication of the character usually employed on railways this did not hold, and the friction was practically constant within the range of pressures used. This fact was entirely borne out by laboratory tests, when made under the proper lubricating conditions, such as those described by the Author: the tests referred to by Mr. Mailloux had not been made under these conditions and were therefore not applicable. It was impossible to look to this cause as the explanation of the fact that a loaded goods-wagon was easier to haul than the same wagon empty. The Author submitted that the true explanation was to be found in the difference of the flange-action in the two cases. Mr. Mailloux also dissented from the view that journal-friction was independent of the speed, and referred to the admittedly high value of journal-friction at starting in proof of his contention. The Author was quite aware of the high value of what was called "starting-resistance," and had always taken this into account in calculating the acceleration of trains.[1] The Paper, however, dealt only with the resistance of trains running at a uniform speed, and not with the conditions when starting. The high initial value of journal-friction was maintained only for a few revolutions of the axle, and after that all investigators were agreed that journal-friction was practically independent of the speed. It was quite true that, as Mr. Mallock had pointed out, in estimating the amount of track-resistance, the Author had taken no The Author.

[1] See C. A. Carus-Wilson, "Electrodynamics," p. 176. London, 1898.

The Author. account of the resilience of the track. It was difficult to estimate how much of the work done in compressing the track was given back in consequence of its resilience, although it was certain that an ordinary railway-track was very far from being perfectly resilient, as Mr. Mailloux had observed, and the Author had intentionally assumed that none of the work was returned. He had shown that track-resistance was negligible in the case of long trains, and need only be taken account of with single coaches or motor-coaches with one or two trailers. If the track was actually to any practicable extent resilient, the effect would be still further to reduce the amount of this item of resistance. In discussing rolling-resistance Mr. Mailloux had objected to the Author's use of the term "rolling-friction." The Author would point out, however, that the rolling-resistance dealt with by Mr. Mailloux was not the same thing as the rolling-friction referred to in the Paper. Rolling-friction was defined in the Paper as "the resistance to the motion of a wheel rolling on a clean smooth rail arising from the elastic indentation of the rail," the resistance offered being due to "the consequent friction as the rail rubs over the surface of the wheel in its endeavour to regain its normal level." The term friction was quite accurately attached to this action, and was not a misnomer, or misleading, although it might be attributable to the last century, and was due, as stated in the Paper, to Professor Osborne Reynolds. But Mr. Mailloux had overlooked the limitation involved in the definition—a wheel rolling on "a clean smooth rail," which the Author considered was a fair description of the conditions existing on a main line of railway in first-class order, the conditions, in fact, which were assumed throughout the Paper. Under such conditions, rolling-friction was, as the Author had shown, a negligible quantity when compared with journal-friction. Now the rolling resistance dealt with by Mr. Mailloux was the resistance offered by sand, mud, dust, refuse and dirt as commonly found on a street-railway track, under conditions totally different from those considered in the Paper. The Author was aware that these conditions, coupled with others peculiar to street-railways, resulted in greatly augmenting the tractive resistance, but he had confined his remarks in the Paper exclusively to dealing with ordinary railway conditions, and had made no attempt to take up the question of the resistance to traction on street-railways, which would involve the introduction of entirely new factors, such as the rolling-resistance dealt with by Mr. Mailloux. He could say, however, that rolling-resistance alone was not enough to account for the increased resistance of street-railways, since when the tracks were thoroughly clean and free from

dirt and obstruction, the tractive-resistance was still far greater than that on an ordinary railway. The question was admittedly one of great interest and importance, but it should not and need not be mixed up with the question of train-resistance on railways. Mr. Taylor had raised an interesting point when alluding to the practice of some engineers in India, of keeping one rail of the track slightly lower than the other, to ensure steady running. Undoubtedly this would tend to reduce the oscillation, but it would probably produce uneven wear of the flanges, which was undesirable from many points of view. With regard to Mr. Wolley-Dod's remarks, the Author had purposely avoided in the Paper going into the question as to the character of the friction set up by flange-action, as it involved a number of considerations of a complicated nature that were foreign to the main question at issue: he had contented himself with showing that flange-action produced a lateral pressure on the rail, causing a loss of energy in friction. The explanation offered by Mr. Wolley-Dod was, in the Author's opinion, correct in as far as it attributed the frictional loss to a certain sliding that took place, but not as regarded the nature of that sliding, which was much more complicated than Mr. Wolley-Dod supposed, though its precise character did not affect the general conclusions drawn in the Paper as to the effect of flange-action. With regard to observations by Mr. Mallock, Mr. Sayers, and Mr. Wolley-Dod on the subject of flange-action, the Author had made no endeavour to calculate the amount of such action: his object had been to show that the resistance caused by flange-action was proportional to the velocity, to the clearance, and to the mass, and inversely proportional to the wheel-base, and this proportionality was not affected by the length of time during which the flange was inclined to the rail. The actual amount of the action was determined by the results of tests on trains in motion, the continually varying position of the flange with respect to the rail being, as it were, integrated and summed up in the effect on the draw-bar. Mr. Mallock and Mr. Sayers had also referred to the influence of coning the wheels on the resistance. Coning had not so great an influence as was often imagined. For example, in the wheels shown in *Fig. 11*, giving the section used on the Bengal-Nagpur Railway, the tires were coned to an inclination of 1 in 20, while the total clearance between flange and rail was $\frac{5}{8}$ inch. Hence the maximum possible radial difference was 0·0313 inch, or about 0·1 per cent. of the diameter. The mean effective difference would be a small portion of this maximum, making the resistance due to such slipping a negligible quantity. Mr. Mailloux was mistaken in supposing that the Author was not

The Author.

The Author. fully aware of the real meaning of equation 6 (p. 231) relating to air-resistance, a further reference to which would be found in the Author's reply upon the Discussion. The fact that this equation gave the pressure at the centre of the windward side of a large plate, attributed by Mr. Mailloux to Messrs. Finzi and Soldati, had been well known in England since the publication of Dr. T. E. Stanton's researches.[1] The values given in the Paper by the Author for air-resistance encountered by trains were not, however, in any way derived from this equation, but from the results of actual experiments on trains in motion, namely, the experiments made by the St. Louis Electric Railway Test Commission and by Mr. Barbier on the Northern of France Railway. Mr. Mailloux appeared to disregard these in preference for those conducted by Professor Goss, whose results he had adopted in the train-resistance formulas he himself made use of. The Author had for long been well acquainted with the experiments of Professor Goss. These were made on models of flat-ended box cars, the actual dimensions being $12\frac{1}{16}$ inches long, $3\frac{3}{8}$ inches wide, and $4\frac{1}{2}$ inches high. The results obtained when estimated in pounds per square foot of exposed cross section at 60 miles per hour were:—For the pressure in front, 3·75 lbs., for the suction behind, 0·55 lb., making together 4·30 lbs. The results of the St. Louis tests made on a car in motion showed that the pressure in pounds per square foot of cross section at 60 miles per hour on a leading vestibule with a flat profile was 8·20 lbs., with a suction of 0·50 lb. on the rear vestibule, making a total of 8·70 lbs., or more than double the value obtained by Professor Goss. The results for vestibules of other profiles were given in Table I in the Paper, showing that with an actual car in motion the value 4·30 corresponded with a profile between parabolic and standard shape, or was 27 per cent. less than that due to a vestibule of standard profile. It thus happened that the application of Professor Goss's values to vestibules of standard profile, although obtained from and strictly applicable only to flat profiles, was not attended with more than this amount of error. The Author preferred to use results obtained by testing coaches in motion rather than those deduced from experiments on small stationary models. With regard to side resistance, Professor Goss had ascertained that the force on each intermediate model in pounds was given by the equation $F = 0{\cdot}000010\ V^2$, the speed being in

[1] "On the Resistance of Plane Surfaces in a Uniform Current of Air." Minutes of Proceedings Inst. C.E., vol. clvi, p. 78.

The Author.

miles per hour. The exposed area of each model, that was, the roof and two sides, the bottom being screened from the air, amounted to 149 square inches, making the drag due to air-friction equivalent to 35 lbs. per thousand square feet of exposed area. Now Mr. Barbier's tests, as shown in the Paper, indicated a drag equal to 79 lbs. per thousand feet, or more than double that obtained by Professor Goss. The explanation of the difference was obvious. Mr. Barbier's tests were made on ordinary railway-coaches whose sides and tops were covered with irregularities due to fittings, mouldings, etc., resulting in greatly increased friction. The models employed by Professor Goss were made of smooth painted tin, which offered very little frictional resistance, amounting to only 35 lbs. per thousand square feet, a figure only 17 per cent. higher than that observed in the pneumatic-despatch tubes of New York, which had their inside surfaces carefully machined. It was clear that such a low value could not be taken as applicable to ordinary railway-trains. Since with a long passenger-train the side and top friction constituted the most important element of air-resistance, the error in using the values obtained in the experiments with the tin models would be very large.

SECT. II.—OTHER SELECTED PAPERS.

(*Paper No. 3741.*)

"Diurnal Atmospheric Variation in the Tropics, and Surveying with the Aneroid."

By THEODORE GRAHAM GRIBBLE, M. Inst. C.E.

THE regularity of the changes in atmospheric pressure in tropical countries makes it possible to use the aneroid barometer, without a separate recording barometer, as an instrument of precision. It is therefore of much greater value in the tropics than in the temperate zones: on the other hand, diurnal variation in tropical countries is much more pronounced, and a knowledge of this phenomenon is essential to accurate work.

Although it is fully described in meteorological works, diurnal variation in the tropics, as far as is known to the Author, is not dealt with in works on surveying. The phenomenon has been much discussed, but its causes are not known. It exists in all countries, and consists of a so-called "12-hour wave," i.e., two distinct undulations every 24 hours, the night wave reaching its maximum and minimum about 10 P.M. and 4 A.M. respectively, whilst the maximum and minimum of the day wave occur about 9 A.M. and 3.30 P.M. respectively.

In England, the ranges of the night and day waves average about 0·3 and 0·6 millimetre: they are, however, practically undistinguishable during any one day's observation. Only by the harmonic analysis of a whole year's observations can the curves be traced. The reason for this obscuration of the diurnal variation lies in the extreme irregularity of atmospheric pressure arising from cyclonic and anti-cyclonic conditions which greatly exceed the regular daily variation.

The aneroid may be successfully employed for surveying purposes in the temperate zones, if a self-registering barometer is kept at the base-station of the survey and the readings in the field are booked, together with the time of reading. Self-recording barometers, both on the aneroid and mercury principle, are made for measuring altitudes up to 16,000 feet, and are not more expensive than a good plain surveyor's aneroid. The recording instrument and the working aneroid should be standardized together at Kew, and the

respective index errors tabulated. Without the assistance of a recording instrument, the aneroid is of limited use in the temperate zones, and even with its help accurate results are more difficult and tedious to obtain than in the tropics.

Within the tropical zone, the irregular movements of the barometer are comparatively slight. Between 10° north and south of the equator cyclones are almost unknown. Strong winds and violent rains produce extremely small effects upon the barometer. There is a gradual rise in the mean height during the dry monsoon, and a corresponding fall during the wet monsoon, but the movement is slow and the variation small.

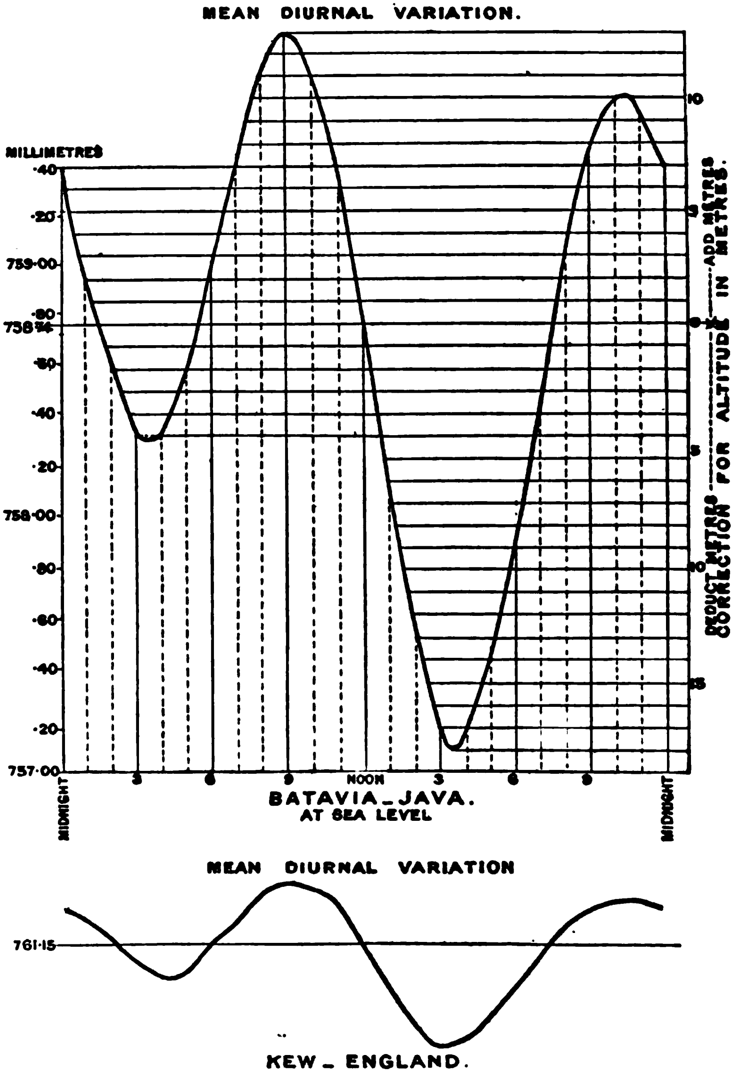
In Java, which is about 7° south of the equator, the "12-hour waves" have an average range at sea-level of 1·34 millimetre at night and 2·87 millimetres during the day, or about 4½ times that of England. Batavia, the capital of Java, possesses a meteorological station of the first rank and of high reputation. The records are published in English and are supplied to the Meteorological Office at Westminster. Hourly readings have been taken at Batavia for 40 years. The following Table gives the mean of 35 years of hourly

Month.	Mean Height of Barometer.	Range of Variation.	
		Night Wave.	Day Wave.
	Millimetres.	Millimetre.	Millimetres.
January	758·70	1·52	2·77
February	758·75	1·52	2·82
March	758·65	1·38	2·92
April	758·27	1·36	2·92
May	758·34	1·34	2·77
June	758·70	1·21	2·62
July	759·01	1·10	2·74
August	759·12	1·19	2·98
September	759·29	1·18	3·16
October	758·82	1·34	3·07
November	758·62	1·45	2·91
December	758·51	1·50	2·76

readings at Batavia, and in *Figs. 1* these are represented graphically and compared with the mean of the readings at Kew during 1905. *Figs. 2*, drawn to a smaller scale, show the mean of the hourly readings during the month of February, 1905, at Batavia and at Kew. When only the hourly readings of a single day are taken, the "12-hour waves" at Kew are undistinguishable, but those at Batavia are usually

almost as regular as the monthly mean. The mean height of the barometer varies in England about fifteen times as much as in Java.

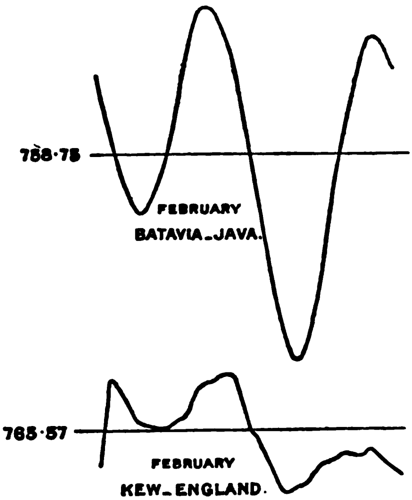
Figs. 1.



Diurnal variation is also affected by altitude above sea-level. In the temperate zones, it varies both in range and phase; in the tropics only in range. In all cases the range decreases with the altitude

Figs. 3 and *4* show curves made in Java by the Author from hourly readings taken at 416 metres and 730 metres above sea-level. The range decreased in approximate proportion to the altitude. If this decrease be maintained in equal proportion, the waves would cease at an elevation of about 2,500 metres. In Madras, however, in

Figs. 2.



latitude about 13° north, where the diurnal variation at sea-level is approximately the same as at Java, the wave at 2,700 metres above sea-level was found to be only one-half that at sea-level, indicating that in that region the waves continue at an elevation of more than 5,000 metres.[1]

[1] The following record of the above observation is extracted from a standard and recent work by Dr. Julius Hann, "Lehrbuch der Meteorologie," Leipzig, 1906.

MADRAS 13° 4′ N, 77° 9′ E, 7 METRES ABOVE SEA.

Heights in millimetres above or below the mean height.

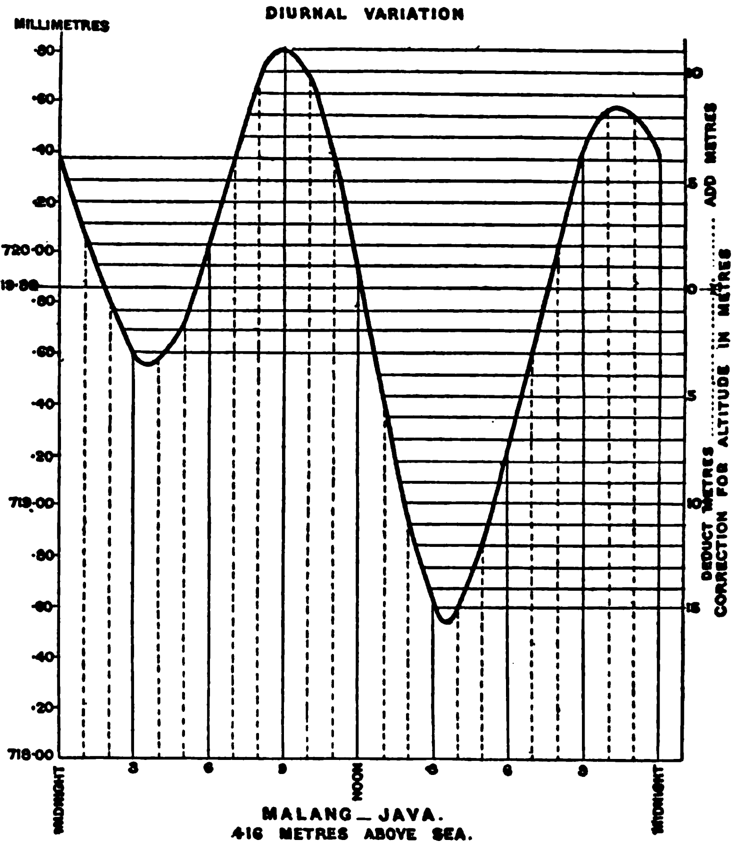
M.N.	2	4	6	8	10	N.	2	4	6	8	10	M.N.
0·45	−0·29	−0·57	−0·02	1·15	1·49	0·57	−0·82	−1·56	−1·13	−0·04	0·73	0·75

DODABETTA PEAK 11° 24′ N, 76° 8′ E, 2,631 METRES ABOVE SEA.

M.N.	2	4	6	8	10	N.	2	4	6	8	10	M.N.
0·12	−0·55	−0·82	−0·34	0·52	1·02	0·55	−0·29	−0·62	−0·34	−0·22	0·54	0·48

In consequence of the fact that in the tropics the phase is unchanged at different altitudes, it is a simple matter to construct a curve of local diurnal variation at any altitude. The upper curve in *Figs. 1* may serve as a standard curve for the tropics. The range of local variation can be found anywhere by observations of the night wave at 10 P.M. and 4 A.M., or of the day wave at 9 A.M. and

Fig. 3.

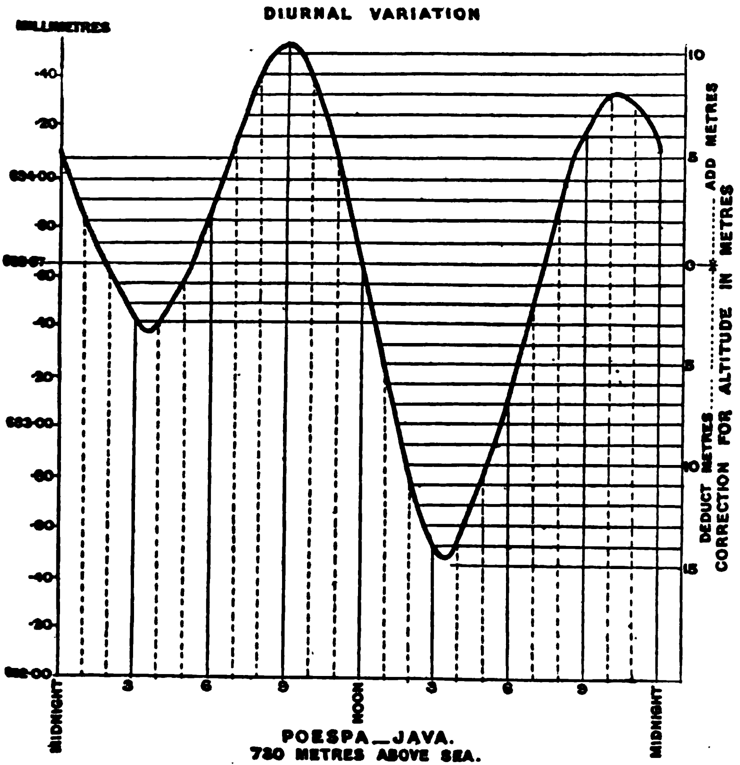


3.30 P.M. It is more satisfactory to take hourly readings and to plot the complete curve, but if it be necessary to commence work on arriving at the station, the curve can be drawn with sufficient accuracy for use and afterwards corrected. The range being determined, the local curve can be constructed from the standard curve, by using proportional compasses or by means of the slide-rule.

The scale of corrections for altitude must, of course, be drawn in each case independently, because the value in decimals of a millimetre of pressure for each metre of altitude varies with the altitude. The altitude scale most generally used by English instrument-makers is that of Sir George Airy, which is shown in the Table on p. 326.

Taking as an example the curve shown in *Fig. 3* at Malang in Java, where the mean height is 719·88 metres, the Table gives a

Fig. 4.



value for 1 metre of altitude in terms of atmospheric pressure of 0·087 millimetre. The diurnal wave range was found to be 2·27 millimetres, therefore the total range was equivalent to 26 metres, from which value the altitude scale was easily constructed.

As will be seen from *Figs. 1*, the range of the day wave in the tropics at sea-level is equivalent to over 100 feet of altitude. During some parts of the day, the change in a quarter of an hour is

Height.	Barometer.	Height.	Barometer.	Value of 1 Metre in Terms of Atmospheric Pressure.
Feet.	Inches.	Metres.	Millimetres.	Millimetre.
0	31·000	0	787·393	
				0·09433
250	30·717	76·20	780·205	
				0·09367
500	30·436	152·40	773·068	
				0·09233
750	30·159	228·60	766·032	
				0·09200
1,000	29·883	304·80	759·022	
				0·09050
1,500	29·340	457·20	745·229	
				0·08883
2,000	28·807	609·59	731·691	
				0·08733
2,500	28·283	761·99	718·382	
				0·08567
3,000	27·769	914·39	705·326	
				0·08333
4,000	26·769	1,219·19	679·927	
				0·08042
5,000	25·804	1,523·99	655·416	
				0·07742
6,000	24·875	1,828·78	631·819	
				0·07467
7,000	23·979	2,133·58	609·061	
				0·07117
8,000	23·125	2,438·38	587·370	
				0·07025
9,000	22·282	2,743·17	565·958	
				0·06692
10,000	21·479	3,047·97	545·562	

equivalent to a difference in altitude of 5 feet, and at such times, it is literally possible to tell the time of day by the height of the barometer.

In most of the tropical countries visited by British surveyors, there are meteorological stations whose records are open to inspection at the Meteorological Office in Westminster, where much other useful information and advice can be obtained by the engineer having to undertake surveying work abroad.

The Author, in conjunction with two other engineers, carried out some surveys for the utilization of water-powers in Java, during the months of June, July and August, 1907, and a brief description of the methods adopted may possibly be of service to others. The physical conditions were such that to have used an ordinary levelling instrument would have been almost impossible, and even with a

tacheometer and stadia rod the work would have been difficult and slow. The survey embraced points at a difference of elevation of 1,200 feet, the highest point being 2,500 feet above the sea. In some places the ravine consisted of a succession of waterfalls, sometimes over 100 feet in height, and its sides were very precipitous, reaching at places a depth of 500 feet. The torrent bed at many places was formed by a mass of enormous boulders, pointing to the existence in former times of far greater hydrodynamic forces than are now apparent.

The self-imposed limit of accuracy in the survey was 5 per cent., the object being to ascertain the available power obtainable from the stream, and the site best suited for the hydraulic installation. It is probable that the accuracy obtained did not exceed an error of 1 per cent. The procedure with the aneroid was fivefold:—

A. *The determination of the mean height of the barometer, and the characteristics of diurnal variation at sea-level.*—This was done by taking hourly readings during the week at the nearest point available.

B. *The determination of the elevation above sea-level of the principal station at the highest point of the survey.*—This was ascertained by reading the aneroid before starting, applying the correction for time, and also the difference between the day's mean height and the average mean height of the week's hourly readings. On reaching the upper station, the correction for time was applied, and hourly readings were taken to ascertain the characteristics of the diurnal variation at that point. As it was not of much importance to obtain with great exactness the absolute elevation of the upper station above the sea, the result given by the procedure described was considered accurate enough for the purpose. If greater exactness had been required, the operation could have been repeated.

C. *The determination of the difference of elevation between the upper and lower limits of the survey.*—As a matter of fact, the lower limit was passed on the way to the upper station and recorded. As it was essential, however, to determine this difference of elevation with the greatest possible accuracy, a succession of readings was taken, the aneroid being rapidly carried on horseback to and fro between the upper station and the lowest point of the survey.

D. *The determination of the elevation of certain intermediate fiducial points along the line of survey.*—This was done in order to diminish the number of journeys to the river-bed: the elevation was obtained in each case by taking repeated readings.

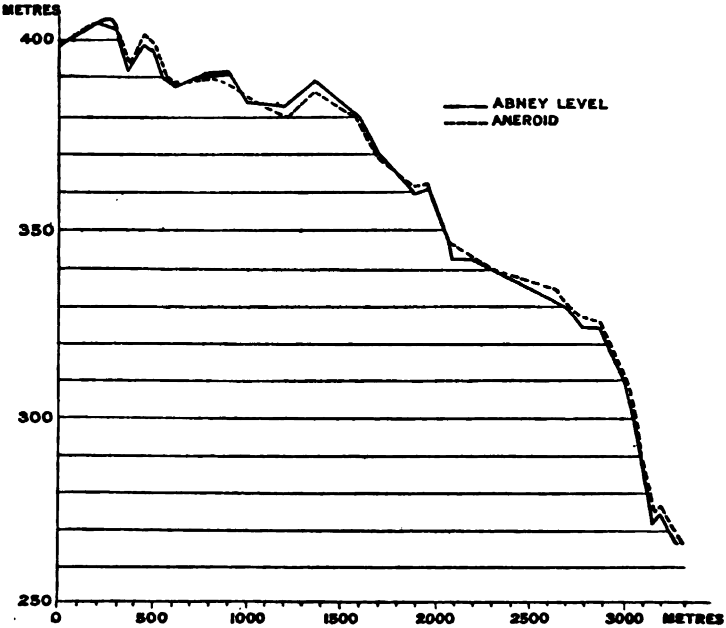
E. *The determination of the actual longitudinal profile of the line of survey.*—The profile was obtained by reading the aneroid at the ends

of each measured base, correcting for time, and checking by reference to the fiducial points as stated under D.

Contoured maps were available, made from Government surveys, which furnished useful checks and supplied the topography of the surrounding country, but they were of no use for the river-bed itself.

The aneroid work was supplemented and checked by the Abney reflecting hand-level. One engineer went in advance, measuring the bases, for which purpose a line of thin rattan was found best,

Fig. 5.



odd distances being taped. At the waterfalls, a special rattan line was dropped from the top of the fall, and so carried by the current to the last station at the bottom of the fall, where the end was cut off and the remainder measured. The second engineer carried a prismatic compass for taking the alignment, which was done by backsight and foresight. He carried also an Abney reflecting-level, for measuring the vertical angle of the base, and was accompanied by two rod-men, having bamboo rods provided with disks, placed at the height of the observer's eye. This operation was also done by back-

sight and foresight. The side-slopes of the sides of the ravine were taken by this engineer. The third engineer carried the aneroid, and also made the sketches.

Fig. 5 shows the profile of one of the surveys. The plan, plotted by means of "latitude and departure," needs no explanation; the profile, however, will serve to show the respective functions of the aneroid and the Abney reflecting level. The full line gives the results arrived at by the Abney level, and the dotted line indicates those obtained by the aneroid.

The final difference of 2 metres between the two instruments was checked by repetitions of the readings at various stations on the way homeward. In each case the aneroid, when corrected for time, read 2 metres higher than when used on the survey. It was therefore assumed that the final difference represented a uniformly increasing error due to irregular variation, and it was so treated.

The intermediate difference between the results of the two instruments was due to tardy or irregular action of the aneroid, mainly arising from the rapidity of the survey. Even the best aneroid procurable is defective in registering any great multiplication of movement, such as occurs between the metallic vacuum and the index hand, and is, therefore, prone not only to tardiness in its response to fluctuations of atmospheric pressure, but also to other irregularities. The hand-level, on the other hand, requires some skill in its use, and is always liable to cumulative error. By always taking backsight and foresight and the mean of the two readings, both index error and personal error are to a great extent eliminated in the Abney level, but in general it may be said that the final result is more correctly given by the aneroid, and the intermediate differences by the hand-level.

The whole survey, of which *Fig. 5* is the profile, was completed in a day. It included alignment by means of the prismatic compass; side-slopes for determining the contours, together with sketches of the intervening features; and photographs of the falls. The work illustrated on the sketch was only about one-tenth of the total amount of survey completed in the manner described, but in every case the agreement between the aneroid and the hand-level was found to be as close as in the profile illustrated.

The Paper is accompanied by five tracings, from which the Figures in the text have been prepared.

(*Paper No. 3652.*)

"Bond in Brickwork."

By Ernest Alfred William Phillips, Assoc. M. Inst. C.E.

It is with diffidence that the Author ventures to reopen a question by common judgment so fixed and settled in theory and practice as correct bonding in brickwork. The Author's practical experience of brickwork dates from 1870, when he received lessons in the art, lasting with intervals about 12 years, from an English master mason and foreman bricklayer and plasterer of over 30 years' experience. The results of the experience thus gained and since extended are submitted in this Paper, and the Author hopes that before criticizing them, the reader will first work out each bond illustrated and described with actual dry bricks or with wooden models of bricks. In no other way does it seem possible to arrive at correct conclusions.

The main object in brickwork is to put the bricks together with good mortar in such bond and manner that, when the work is set, the full strength of both bricks and mortar are utilized.

Modern or so-called English bond is illustrated for walls from 1 to 2½ bricks in thickness in *Figs. 1.* True or old English bond is illustrated in like manner in *Figs. 2.* The two bonds are compared, in general plan, in elevation, and in cross and longitudinal sections, in *Figs. 3.* It may surprise many to learn that the modern form of bonding is a bastard bond; being weak, often ill-jointed, and possessing at the best of times no greater strength longitudinally than the despised Flemish bond, while in thick walls it may become actually dangerous.

Definition of true English Bond.—It is first necessary to lay down a complete definition of true English bond [1] :—

(*a*) The courses are to be composed entirely of headers or of stretchers.

(*b*) Every brick in a header course must therefore be a header, and every brick in a stretcher course must be a stretcher. A mixture of headers and stretchers in any one course is not English bond.

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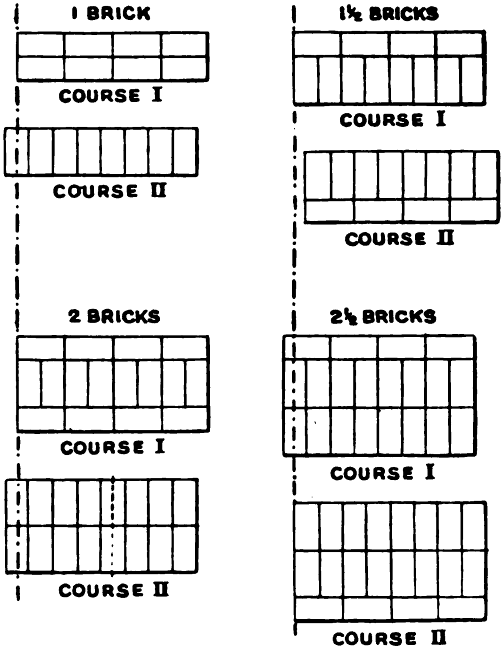
[1] J. Gwilt, "An Encyclopædia of Architecture." New ed. London, 1891.

(*c*) Every brick must break joint in its two longitudinal planes, vertical and horizontal, with all nearest bricks lying in the same parallel direction, and this whether a course of bricks (lying across at right angles) intervenes or not.

(*d*) The bricks must break joint by half their length. In *Figs.* 2, Course I, stretcher A breaks joint by half its length, not only with adjacent stretchers B, B, B, B in the same course, but also

Figs. 1.



BASTARD BOND

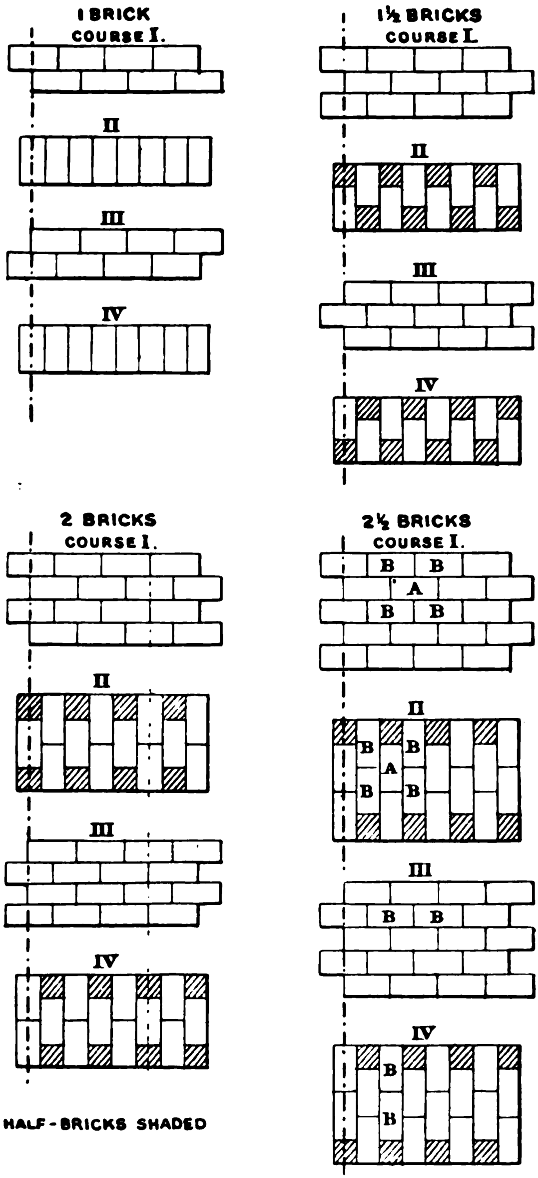
with nearest parallel bricks B, B in Course III and the similar course below. See also Courses II and IV for header bond.

(*e*) Quarter-brick laps are inadmissible, except between headers and stretchers in adjacent header and stretcher courses, and occasionally, for obvious reasons, at quoins and junctions.

(*f*) Lines of stretchers must, therefore, lie exactly over lines of stretchers, and lines of headers over lines of headers.

(*g*) At quoins and junctions the stretcher courses of the one wall will pass through and become the header courses of the meeting or cross wall.

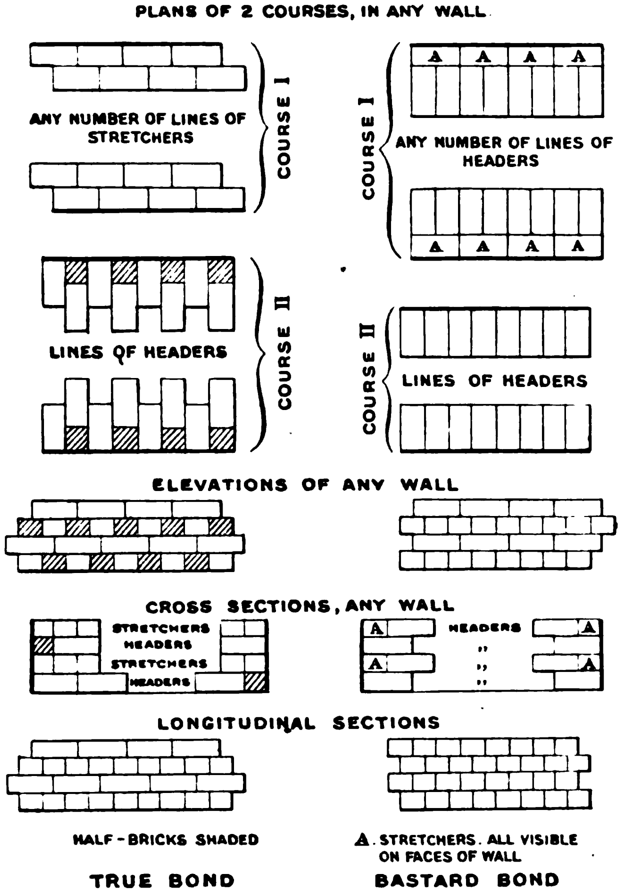
Figs. 2.



TRUE ENGLISH BOND

(*h*) True or old English bond is a fixed and immutable bond, absolutely unchangeable, of complete stretcher courses, repeated as often as desired, and of complete header courses, sandwiched between the rows of stretcher courses.

Figs. 3.



Modern and True Bond compared.—The differences between English bond and bastard bond may be stated as follows:—

(i) English bond consists of alternate or of periodic courses of headers and stretchers, no header course containing any stretchers, and no stretcher course containing any headers.

(ii) In bastard bond every brick in the interior of any wall, however thick, is a header.

(iii) In English bond the composition and arrangement of every course show clearly on the faces of the wall.

(iv) In bastard bond all the stretchers in any wall, however thick, show on the faces of the wall, serving only to conceal the headers inside. (Since the "stretchers" in bastard bond serve merely to "set back" the headers behind them by half the length of a brick, they might more appropriately be termed "fillers.")

(v) In English bond, in any thickness of wall, for any given length and height, the number of half-bricks or false headers in the wall is a constant number.

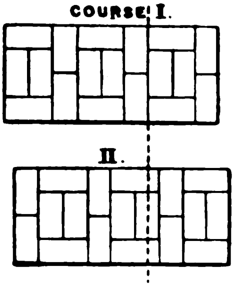
(vi) In bastard bond the number of "fillers" or "stretchers" is also a constant. In each case, English or bastard, in any wall 4 bricks long and 4 courses high, the constant number of half-headers or of "fillers" is 16; in two courses, 8.

(vii) In English bond the overlaps are half a brick, and are formed at every possible opportunity, to secure strength by adhesion of mortar to sides as well as to flats of bricks. This is not the case in bastard bond, where flats alone are considered.

Fig. 4.

2 BRICKS

COURSE I.



FLEMISH BOND.

(viii) In true bond the stretcher bricks break joint on the faces of the walls. In bastard bond the stretcher joints fall vertically over one another, from base to summit of walls (*Figs. 3*).

Walls built in bastard bond seem from their faces to be built in alternate courses of headers and stretchers, but the similarity extends no deeper than the faces, every stretcher showing on the faces, and every other brick in the wall being a header in a mass of other headers. In thick walls, as the Author is aware from experience, it is easy to lose the quarter-brick laps, in which case the longitudinal tie for many courses in succession might depend solely on the strength of the mortar joints. From a practical bricklayer's point of view, quarter-brick laps are not always reliable. With the rule of half-brick laps, a quarter-brick actual lap is a practical certainty.

Trial by number of Stretchers v. *Headers.*—Gwilt gives an example of a piece of walling, 4 bricks in length, 2 bricks thick, and 4 courses high. In English bond (*Figs. 2*) such a piece of brickwork would contain 32 stretchers, 24 whole-headers, and 16 half-bricks, or false headers. In Flemish bond (*Fig. 4*) there are 42 whole-

headers and 20 stretchers. From this example, Gwilt infers the superior strength of English over Flemish bond. Bastard bond, however, for the same wall (*Figs. 1*) would contain 48 headers to only 16 stretchers. These results are here tabulated:—

Bond.	English Two Courses.			Flemish Two Courses.			Bastard Two Courses.		Remarks.
Thickness of Walls.	Stretchers.	Headers.	Half-bricks.	Stretchers.	Headers.	Half-bricks.	Stretchers.	Headers.	
Bricks. 1	8	8	..	12	6	..	8	8	The Flemish wall considered is 4½ bricks long; the other two each 4 bricks long. It takes 4 courses to complete the true English bond; 2 courses each for bastard and Flemish.
1½	12	8	8	12	12	6	8	16	
2	16	12	8	12	24	..	8	24	
2½	20	16	8	12	30	6	8	32	
3	24	20	8	..	..	..	8	40	
3½	28	24	8	..	..	..	8	48	
4	32	28	8	..	..	..	8	56	
4½	36	32	8	..	..	..	8	64	
5	40	36	8	..	..	..	8	72	
5½	44	40	8	..	..	..	8	80	
6	48	44	8	..	..	..	8	88	

In comparing the numbers for each bond in the Table, it should be remembered that while the laps in English bond are all half-brick laps, the laps in bastard and Flemish bonds are quarter-brick laps. Bastard bond in thick walling is really "header bond." [1]

Trial by Carpentry.—A carpenter is given a number of wooden blocks, say 9 inches by 4½ inches by 3 inches thick, with instructions to form them into a narrow deep beam, to be laid on ordinary ground with a view to distributing loads placed along the top of the beam. He is to be allowed plenty of marine or other strong glue, but no nails or dowels, and he may only cut the blocks in the same manner that bricks are shaped in ordinary brickwork. If the man considers the matter carefully, he will probably employ the true or old English form of bonding, because it gives the glue the best and completest chance of doing its work of holding the blocks together.

[1] See Note to Appendix.

Rankine's Theory of Strength of Brickwork.—Rankine gives[1] calculations for the strength of bond. He ignores the strength of the mortar, and relies on the frictional tenacity of the horizontal joints, the mortar being held to be green and unset. His calculations refer without doubt to an imperfect English bond, completed for header courses only, and but half completed for the stretcher courses, since he shows mathematically that "English" bond is only half as strong longitudinally as transversely. If his method of calculation be applied to the true or old English bond (*Figs. 2*) of alternate courses of headers and stretchers, the longitudinal strength will prove to be a little greater than the transverse strength in thin walls (owing to the presence of the half-bricks), while in very thick walls the strength both ways will be found to be equal. It is quite unnecessary, in true or old English bond, to employ two courses of stretchers to one of headers to obtain equal strength longitudinally and transversely. In this theory it is taken for granted that, however old or well-set the work may be, the mortar must never be depended upon to hold the bricks together. The strength of the bonding is calculated almost as if the wall were built with dry bricks, the coefficient of friction, f, being assumed to be the coefficient for brickwork laid in damp mortar, "which has not had time to acquire appreciable tenacity."

Whilst it cannot be denied that it is sound practice in calculating the stability of structures to ignore the tenacity and adherence of mortar, and to design as if resistances to loads and pressures were obtained solely by weight, mass, and friction, it seems to the Author to be unreasonable to ignore the mortar when calculating the internal strength of masonry or of brickwork. Concrete, for instance, depends for its very existence on the strength of the mortar binding the aggregate. All practical tests of strength of brickwork are made after the mortar is set. It is most unusual, and it is considered bad practice, to load green brickwork. If this principle be conceded, it follows that that form of brick bond is not necessarily the best which gives only the maximum frictional tenacity without reference to adherence of mortar to bricks.

Trial by Frictional Tenacity.—Rankine's calculations apply, therefore, to the case of bastard bond, which is one-half as strong longitudinally as transversely. Reliance being placed only on "frictional tenacity," no attempt is made to secure overlaps except in vertical planes, that is, in both sections. In plan, the bricks do

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[1] W. J. M. Rankine, "Applied Mechanics." 5th ed., p. 222. London, 1896.

not break joint.[1] For all practical purposes, according to this theory, it is scarcely necessary that the vertical joints be filled with mortar. Yet the filling of the vertical joints is most necessary if adequate strength against shearing is to be secured.[2] Assuming that nothing but frictional stability is called into play, the bastard bond is perfect. But when other stresses occur, due perhaps to unequal settlement or to earthquake, it is found in practice that walls in bastard bond crack nearly vertically from top to bottom, and in every such case it will, on investigation, be evident, without doubt, that the crack or split was the result of a comparatively slight cause, and was due primarily to the inherent longitudinal weakness of the bastard bond.

Applying Rankine's theory to the true bond, it will be found that the half-brick overlaps secured in transverse bonding are also fully secured in longitudinal bonding. English bond is, therefore, at least as strong longitudinally as transversely.

Trial by Adhesion and Strength of Mortar.—Admitting that mortar will set in due time, that it will adhere to bricks, that no loads will be applied until the brickwork has had reasonable time to settle and to set; it follows that the best form of brick bonding is that which gives the mortar the best chance of holding the bricks together, and by which also the maximum frictional tenacity is obtained, whether the mortar be set or green. Of all possible forms of bond, the only bond which fulfils the above two conditions is true or old English bond. In this bond, which for thick walling cannot be improved upon, "tie" is secured in two ways. It contains not only the maximum number of half-brick overlaps on the flats of the bricks, offering resistance both by adhesion and by friction; but also, the maximum number of half-brick side laps, offering resistance by adhesion of mortar only.

With bricks, including joints, averaging 9 inches $\times$ $4\frac{1}{2}$ inches $\times$ 3 inches, and correct half-laps, the area covered on the half-side of a brick is $4\frac{1}{2}$ inches $\times$ 3 inches $= 13\frac{1}{2}$ square inches. On the half-flat it is $4\frac{1}{2}$ inches $\times$ $4\frac{1}{2}$ inches $= 20\frac{1}{4}$ square inches. In old English bond, the longitudinal bond is perfect. Owing to the half-headers, the transverse tie in $1\frac{1}{2}$ and 2-brick walls is 1 brick, or $2\,(13\frac{1}{2} + 20\frac{1}{4}) = 67\frac{1}{2}$ square inches of adhesion. In $2\frac{1}{2}$ and 3-brick walls it is 2 bricks, or 135 square inches of adhesion. In $3\frac{1}{2}$ and 4-brick walls it is 3 bricks or $202\frac{1}{2}$ square inches of adhesion, and

[1] In Kempe's *Engineer's Year-Book*, 1906, p. 892, it is stated that "No stretchers occur, except those seen on faces of walls. No bricks in the same course should break joint with each other."

[2] Minutes of Proceedings Inst. C.E., vol. clxi, p. 330.

so on. The thicker the walls, the less the difference in strength, in longitudinal and transverse ties, in old English bond.

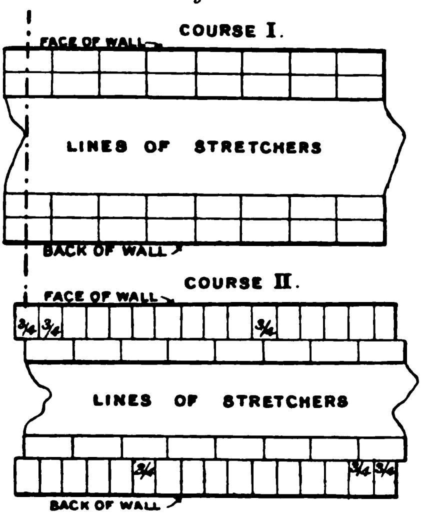
In good work, carefully laid, bastard bond has a longitudinal tie due to quarter-brick laps. In a 2-brick wall this is 2×9 inches $\times 2\frac{1}{4}$ inches $= 40\frac{1}{2}$ square inches, as against $3 \times 4\frac{1}{2}$ inches $\times$ 3 inches $+ 4 \times 4\frac{1}{2}$ inches $\times 4\frac{1}{2}$ inches $= 121\frac{1}{2}$ square inches in old English bond. In a $1\frac{1}{2}$-brick wall, the comparison is 1×9 inches $\times 2\frac{1}{4}$ inches $+ 1 \times 4\frac{1}{2}$ inches $\times 2\frac{1}{4}$ inches $= 30\frac{3}{8}$ square inches as against $2 \times 13\frac{1}{2} + 3 \times 20\frac{1}{4} = 87\frac{3}{4}$ square inches. In a $2\frac{1}{2}$-brick wall it is $50\frac{5}{8}$ to $155\frac{1}{4}$ square inches; and in a 6-brick wall, $121\frac{1}{2}$ to $391\frac{1}{2}$ square inches. The thicker the wall, the better the comparison in favour of the old English bond. Taking no account of the quarter-brick laps in header courses (as effective in old English as in bastard bond), the true English bond is at least three times as strong longitudinally as the bastard bond; or four times as strong, if the quarter-brick laps in old English bond are allowed for and added. The cross-tie in bastard bond is due to half-brick laps on flats of bricks only. Four courses must be considered as against two, so that bastard bond is 20 per cent. stronger transversely than old English bond, this result being attained, however, at a great sacrifice of longitudinal strength.

Some text-books enlarge on the importance of laying bricks so that every vertical joint in the course immediately below may be correctly covered. In the case of a thick wall, any arrangement of bricks with this end in view must, it is believed, fail; unless the bricks are of perfect shape and exact dimensions, and highly skilled bricklayers are employed. Quarter-brick laps alone being possible, the practical objection already urged against such small laps holds good. With a rule of half-brick laps, it may be reasonably expected that one-third, and at the worst one-quarter laps, will be secured. With a rule of quarter-brick laps, laps of from 1 inch to nothing, in the body of a wall, are probable, especially with careless or inexperienced bricklayers. If the argument be admitted, however, that quarter-brick laps can be always correctly laid, attention is invited to the reversed bastard bond illustrated in *Fig. 5.* The longitudinal strength of this bond is to the longitudinal strength of old English bond as 6 : 5; the transverse strength as 1 : 4. It may be asked, are engineers prepared to employ such a bond as this reversed bastard bond in all or any of their works? Yet this reversed bastard bond is practically the modern bond reversed in a heavy retaining or dock wall; that is, a bond designed for effective resistance in one direction only.

From this reasoning it seems clear that if the mortar is so poor that it possesses no strength or power of adhering to the bricks, all that can be relied on is frictional tenacity. If the mortar possesses any strength at all, and if it will bear any tensile stress, however small, it seems worth while to utilize this strength, and to pay attention to side-laps and to the proper filling of vertical joints in the brickwork.

The Bonding of Thin Walls.—There is little doubt that old English bond is unsuitable for walls of thicknesses between 1½ bricks and 2½ bricks, both inclusive, owing to the presence of half-headers In 1½-brick walls they amount to one-sixth of the whole content of

Fig. 5.

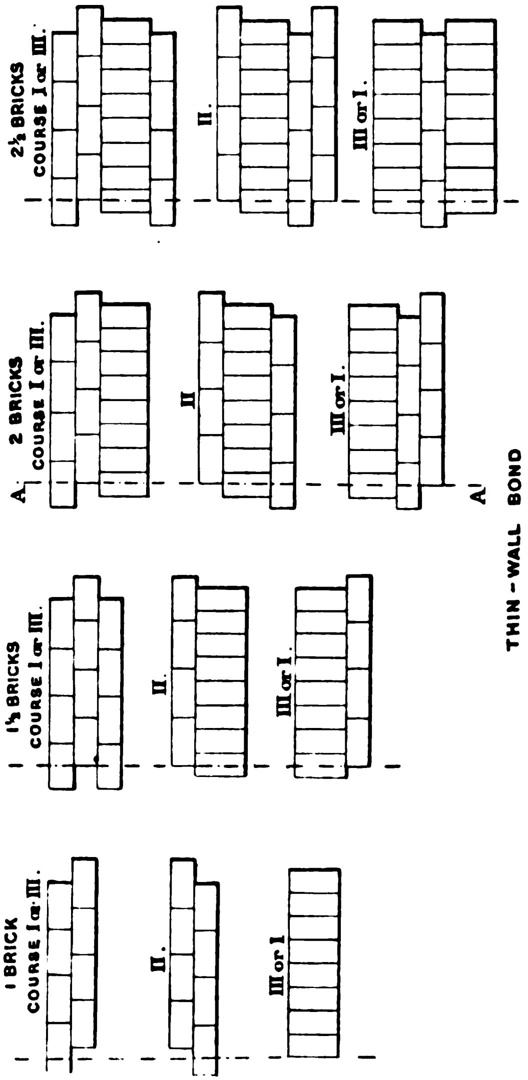


REVERSED BASTARD BOND.

the walls; in 2-brick walls to one-eighth; and in 2½-brick walls to one-tenth. Some other form of bonding must be sought for walls of 1½, 2, and 2½ bricks in thickness, and the Author believes this need is met by employing the "thin-wall bond" illustrated in *Figs. 6, 7* and *8* inclusive. In this form of bonding it takes three courses to complete the bond, and it will also be noticed that while the longitudinal tie is due to bonding similar to the longitudinal bonding of old English bond, the cross-tie is that of the bastard bond. The longitudinal tie in a 2-brick wall is equal to the longitudinal tie of old English bond. The cross-tie in the same wall is two-thirds that of bastard bond, or 80 per cent. of the cross-tie of old English bond, making no

allowance for loss of strength caused by using half-headers. The thin-wall bond loses strength as the wall increases in thickness. It is, however, well adapted for circular walling, as its use does not

Figs. 6.



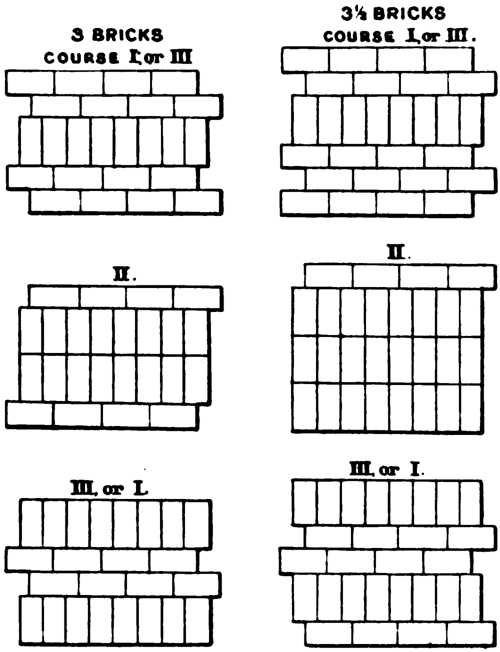
(Note.—In 1 and 1½-brick walls the 1st and 4th stretcher courses to break joint as in true English bond.)

involve the cutting and shaping of bricks, or the employment of specially moulded bricks. The bond changes with every half-brick added to the thickness of the wall, and it is therefore considered

desirable (for use in circular walling) to give details up to 5 bricks thick. An imitation of the bond can be produced by inserting one entire course of stretchers between every pair of courses in bastard bond (Courses I and II) *Figs. 1.* This imitation is shown as "contractor's bond" in *Figs. 9*, but is not recommended for important works, being weaker longitudinally than "thin-wall bond," although less troublesome to lay.

It should be noted that the term "bastard bond" is applied by

Figs. 7.



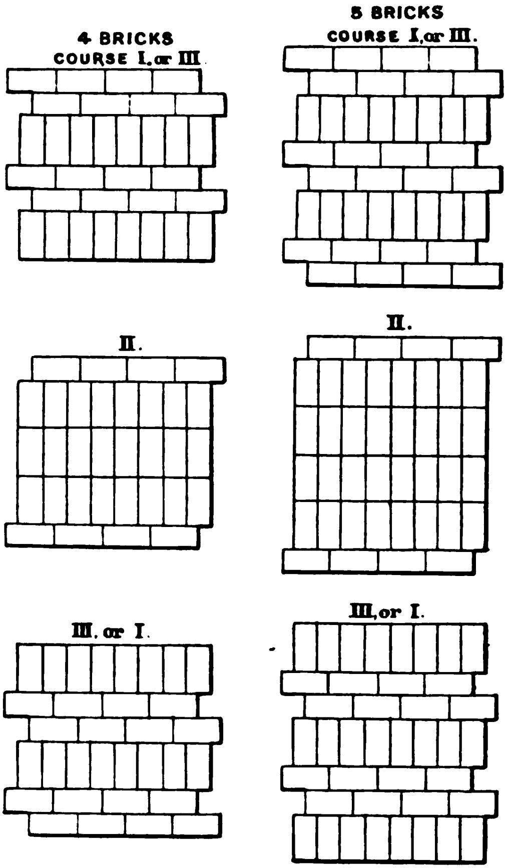
THIN-WALL BOND

practical bricklayers to any form of brick-laying not in a recognized bond. The Author submits that modern English bond is not only based on a wrong theory, but is opposed to true English bond in every important point, and is therefore a bastard bond. He contends that true old English or Roman bond is not merely a class or form of bond; but that it is an absolutely unchangeable bond, and there is only one correct way to lay it and to estimate its strength.

The Author believes that bastard bond is of comparatively recent

origin, dating probably in its present form from between the years 1870 and 1875. The only practical working bonds known to him are: (1) English bond, with its modifications, thin-wall, diagonal, herring-bone, garden-wall, and chimney bond; and (2) Flemish bond. Bastard bond cannot be admitted as a recognized bond, and the

Figs. 8.



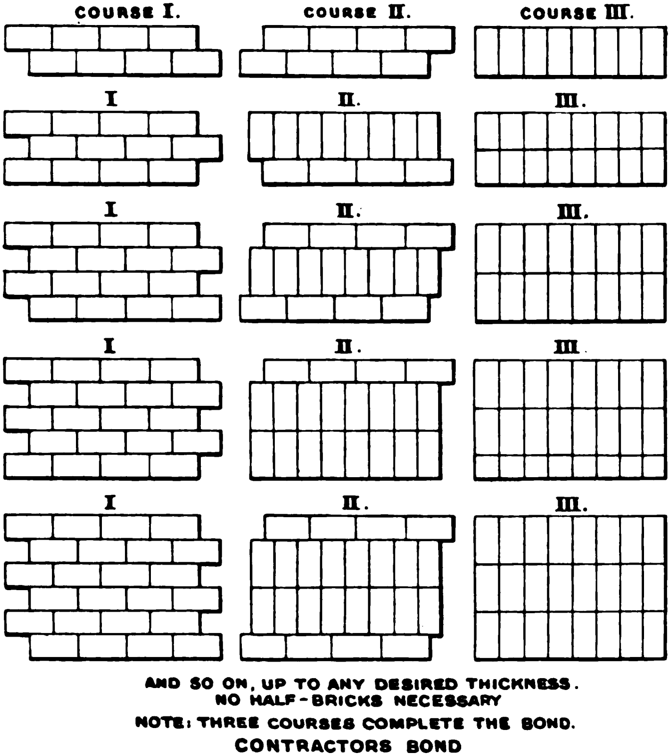
THIN-WALL BOND

Author has never accepted it as such, regarding it as an unsound and unscientific innovation.

Practicability.—The most important point remains to be considered. Many engineers object to a bond of more than two ... being impracticable. It takes four courses to complete

true English bond, by reason of double lapping, sides and flats of bricks. This is inevitable. If work is always closed on a header course, and if the put-log holes are invariably the result of omitting false headers, or half-bricks, as they should be, it is not easy to see how laps or flats in header courses can be lost. Mistakes cannot be made in stretcher courses, as the first row of stretchers in each

Figs. 9.



(Note.—1st and 4th courses to break joint as in true English bond.)

course shows plainly on face of wall, and is a key to the rest and to the courses above and below. Mistakes are impossible in thin-wall and contractor's bonds, as can be shown by models of bricks.

The Paper is accompanied by 18 sheets of diagrams from which the Figures in the text have been selected, and by the following Appendix.

APPENDIX.

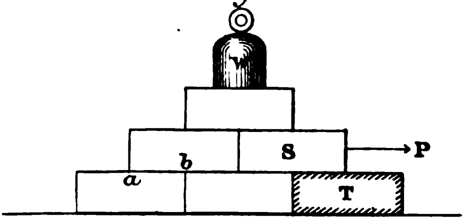
THE STRENGTH OF BRICK BOND.

1. Walls are generally called upon to resist only two extraordinary dislocations, viz., (i) splitting into thicknesses, and (ii) cracking vertically or diagonally through the entire thickness of the wall.

2. Resistances to splitting and cracking depend on the same sources of strength, and may be considered together. The sources of strength are—(*a*) "frictional tenacity" (Rankine, "Applied Mechanics"), and (*b*) the adhesion of the mortar which holds the bricks together. Rankine ignores (*b*) altogether, but nevertheless the adhesion of the mortar to the bricks is a most important consideration in the strength of walls. Rankine, in making his calculations, assumes the mortar to be still soft and unset, with the result that his reasoning is inexact, and gives an unfair basis of comparing the strengths of the various bonds in use.

3. Frictional tenacity is illustrated by the following diagram (elevation) :—

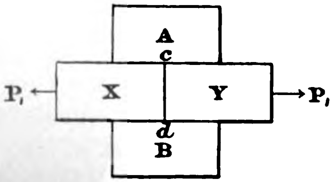
Fig: 10.



One brick rests lengthwise and "dry" on two, which rest "dry" in turn on three, placed on the ground to form a half-brick wall, the laps, *a b*, being half-brick laps, say $4\frac{1}{2}$ inches $\times$ $4\frac{1}{2}$ inches. W represents the superincumbent load, P any force tending to pull the brick S out of place. If the shaded brick T be now removed, the resistance to P is that due to friction on two half-brick laps = friction on areas 2 inches $\times$ $4\frac{1}{2}$ inches $\times$ $4\frac{1}{2}$ inches = $40\frac{1}{2}$ square inches. If S be now considered as cemented into the half-brick wall, the illustration is complete.

4. Adhesive tenacity is illustrated by the following plan :—

Fig: 11.



Here X and Y are firmly cemented together on joint *c d*, measuring say $4\frac{1}{2}$ inches $\times$ 3 inches, and A and B are cemented in turn to X and Y by half-brick laps,

measuring each also $4\frac{1}{2}$ inches by 3 inches. If forces $= P_1$ are now applied to pull X and Y apart lengthwise, the adhesion of the cementing matter to the bricks offers resistances on areas aggregating 3 inches $\times$ $4\frac{1}{2}$ inches $\times$ 3 inches $= 40\frac{1}{2}$ square inches.

5. If a brick is built into a wall in such manner that half its length projects, the power, P, to pull it out must be greater than the combined resistance due to frictional tenacity and to adhesive tenacity, as described above. If the wall has taken its final settlement, and the mortar has thoroughly set, it will be found that the total resistances are probably all due to adhesive tenacity only, that is, due to the adhesion of the mortar to 81 square inches of surface. This is the theory of strength of true or old English bond, and to this end all specifications regarding the wetting of bricks, the keeping damp of brickwork, and the full flushing up of all joints with sound mortar are framed.

NOTE.—The Author has since ascertained that bastard bond is actually a header bond, of the alternate header courses of a form of bond still occasionally used in Sussex, and illustrated and referred to in *Indian Engineering*, vol. xlii (1907), pp. 7, 77 and 124. While true English bond (*Figs.* 2) may be called the English bond of the half-brick laps, "Sussex" bond might be termed the English bond of the quarter-brick laps.

(Paper No. 3738.)

"Flowing-Water Problems."

By Edgar Charles Thrupp, Assoc. M. Inst. C.E.

The Paper relates particularly to the question of the ratio of the mean velocity in a stream to the maximum surface velocity under various circumstances.[1] It includes the results of the Author's studies on the general laws of flow in rivers, which have been briefly indicated before,[2] and are here presented for the first time in the form of a diagram based upon definite formulas; and some data relating to scouring power are also given.

General Laws of Flow and the critical velocity in open channels.— In 1887, in a Paper[3] read before the Society of Engineers, the Author gave formulas and coefficients for the flow of water in pipes and open channels: the formulas are summarized in Table II, and will be referred to later. With regard to large open channels, however, he has discovered that the critical velocity phenomenon occurs at much higher velocities than had been supposed, and his conclusions arrived at in 1887 are therefore superseded by the formulas given in the present Paper.

Fig. 1, Plate 5, is a logarithmic diagram representing the laws of flow in rivers, canals and smooth open channels of fairly uniform section, and approximating in character to smooth earth or mud and sand. The horizontal scale gives the slope expressed in terms of length of channel divided by the fall. The vertical scale is plotted to the values of $4 \pi R^2 \times v$, that is the discharge in cubic feet per second of a pipe of the hydraulic radius R. This quantity has no practical significance in reference to open channel work, but it gives a convenient scale for plotting the diagonal lines representing the hydraulic radii and mean velocities, and other curves shown. The

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[1] See Discussion on Water-Supply Papers by Messrs. Tait, Barnett and Hill. Minutes of Proceedings Inst. C.E., vol. clxvii, p. 216.

[2] Minutes of Proceedings Inst. C.E., vol. cxlvii, pp. 125–128.

[3] "A New Formula for the Flow of Water in Pipes and Open Channels." Transactions of the Society of Engineers, 1887, p. 224.

position of the critical region is clearly indicated by the curious curves representing the hydraulic radius and velocity lines.

Whilst the diagram extends only to a hydraulic radius of 0·10 foot, the position of the critical region probably begins to curve a little above this point, and rapidly assumes an entirely different inclination below it, but further data are needed to locate the curve accurately. For practical work on open channels, however, this doubtful region will rarely be required, and the error, within the limits of the diagram, cannot be large. The lower velocity boundary of the critical region is probably affected considerably by the temperature of the water, and also by the roughness of the channel and by weeds or rushes. The sites of the Author's experiments on the Kennet and the Thames were somewhat obstructed by weeds and rushes, and are therefore not suitable for locating the critical points on a general diagram dealing with unobstructed channels. The Mississippi (1882) data have therefore been used to locate the critical point.

The construction of the diagram in Fig. 1 is based upon the following three formulas:—

(*a*) For slopes of 1 in 10,000 or steeper,

$$v = \frac{R^{0\cdot61}}{0\cdot01256\ S^{0\cdot5}}$$

(*b*) For the middle of the critical region (the tangent lines to the two curves on the diagram),

$$v = \frac{R^{0\cdot61}}{0\cdot1442\ S^{0\cdot25}}$$

(*c*) For slopes of 1 in 100,000 or flatter and up to the critical region,

$$v = \frac{R^{0\cdot7}}{0\cdot000002819\ S^{1\cdot25}}$$

where v = mean velocity in feet per second.
R = hydraulic radius in feet.
S = length of channel divided by the fall.

These formulas apply to the straight parts of the lines in Fig. 1, representing the hydraulic radius and the velocity: the curves rounding off their connections at the sides of the critical region are added without the use of a formula.

It was shown by the Author in 1887 that Kutter's formula for open channels was far less accurate than was generally supposed: that conclusion is now emphasized by the demonstration of the critical velocity phenomenon given in Fig. 1.

Many early writers (Hagen, Gauckler, Kutter and others) were influenced by data showing that the resistance on gradients flatter than 1 in 10,000 varied in some ratio greater than v^2, but they all failed to observe that the range of this law was very limited and that at extremely flat gradients the resistance varied in some ratio very much less than v^2; in fact, more nearly as v, just as it does in very small pipes, but with much steeper gradients, as was shown by Professor Osborne Reynolds in 1883 and by the Author in 1887. The discovery of a wide departure from the Osborne Reynolds law of critical velocity was first made by the Author in experimenting on a 15-inch cast-iron pipe about 10 miles long, and he followed it up by investigations carried out on the Thames and Kennet, taking special precautions to observe the slopes with hook gauges placed in sheltered boxes at points which had been connected by careful hydrostatic levelling operations. The results of these three sets of experiments are given in the Appendix.

Another set of experiments was made on a wooden trough 8 inches wide, with a view to locate the critical velocity for a hydraulic radius of 0·10 to 0·15 foot. These experiments were so far successful as to show that the critical velocity was below 0·20 foot per second, but the trough was not sufficiently long to enable the law of resistance on gradients flatter than 1 in 100,000 to be traced satisfactorily.

On a careful study of the published results of the observations made by the Mississippi River Commission, it became apparent that the data obtained at Carrollton agreed better with the Author's formulas than with any previously given, and showed that the conditions of flow at that site were on the verge of the region where the resistance diminishes in a ratio less than v^2.

The operations carried out at Carrollton in 1882 are the best set of gaugings made there, as regards consistency and apparent reliability. In extensive gauging operations the element of personal error on the part of the staff employed is always liable to produce unaccountable differences, and in some later gaugings the Commission found such wide discrepancies that they decided not to publish the slope observations. On gradients of 1 in 100,000 or thereabouts the difficulty of making accurate slope measurements by ordinary levelling is so great that discrepancies must be tolerated which would not be admissible on gradients of 1 in 10,000 or steeper. It is, however, to be hoped

that more perfect data will be obtained in future on large rivers with hydrostatic levelling hose-pipes immersed in the water, connecting the slope stations, and acting as "pilot tubes" to read the water-levels at the centre of the stream, instead of at the sides as is usually done.

In locating the boundaries of the critical region in Fig. 1, the higher velocity side has been put at about 1 in 10,000 because an examination of hundreds of recorded experiments failed to disclose any substantial evidence of the resistance varying in a ratio greater than v^2 on steeper gradients. Whilst there is ample evidence of a higher ratio between the limits of gradients of 1 in 10,000 and 1 in 100,000, the boundary of the critical region on the lower velocity side does not appear to lie so nearly on a fixed gradient as on the other side, but ranges from 1 in 65,000 with a 1-foot hydraulic radius to 1 in about 100,000, with a 100-foot hydraulic radius.

Ratio between the mean and maximum surface velocities in open channels.—The relation between the mean velocity and the maximum surface velocity in open channels has always been attractive as being a simple factor to be used in gauging, but the accuracy obtainable depends upon a knowledge of the correct ratio to apply in any particular case. Experimental results have been published giving values for this ratio varying between 0·47 and 0·95, but no formula, or table, has ever been published, so far as the Author is aware, which reconciles the discrepancies between various observations or which ensures results correct within 10 per cent. Several observers have constructed formulas to suit the conditions of their own experiments, but these have not given general satisfaction.

Many experiments were apparently vitiated by the presence of weeds in the channels, and a careful examination of the data shows that weeds, rushes, large stones or other obstructions existed in practically every case where a ratio of less than 0·70 was observed. As it is quite hopeless to attempt any mathematical treatment of weedy channels, the problem is narrowed to that for clear channels with variations of between 0·70 and 0·95. Discrepancies of 5 per cent. in a single set of experiments often occur in good work, and even the very valuable work of Darcy and Bazin could not, by itself, furnish a guide to the general conclusion hereafter stated, but before the direction of the velocity-ratio curves could be ascertained, other experiments under widely different conditions had to be made, after which some puzzling differences still remained to be dealt with. From a set of experiments carried out by Boileau on a channel having vertical sides and a considerable range of variation in depth, it was evident that the shape of the

channel affected the result, and traces of the same effect appeared in other sets of experiments. The question arose as to how to express "shape" in a formula. The effect of shape upon the velocity ratio is that in two channels of the same hydraulic radius but of different widths and depths, the narrow and deep channel will show the higher velocity ratio. The hydraulic radius alone does not "standardize" the dimensional properties of channels for the present purpose. After many trials the Author found that a simple and suitable factor for representing "shape" was the ratio of the surface width to the wetted perimeter, or $\frac{\mathrm{W}}{\mathrm{P}}$.

The next point to be settled was a standard form of channel for general comparison, and the choice seemed to lie between a channel of infinite width (giving $\frac{\mathrm{W}}{\mathrm{P}} = 1$), and a semicircular channel (giving $\frac{\mathrm{W}}{\mathrm{P}} = \frac{2}{\pi} = 0{\cdot}63$). The former was adopted and the standard channel velocity ratios corresponding to experimental conditions were calculated in a large number of cases in accordance with the empirical rule:—

$$\frac{\text{mean } v}{\text{max. surface V}} = \text{standard } \frac{v}{\mathrm{V}} \times \left(\frac{4}{3 + \frac{\mathrm{W}}{\mathrm{P}}}\right)$$

The ratios so arrived at from several hundred cases were marked upon diagrams similar to Fig. 1, against dots locating the hydraulic conditions of R and v, and fifteen curves were contoured in "give and take" lines representing the location of the conditions under which the values of the standard $\frac{v}{\mathrm{V}}$ (from $0{\cdot}70$ to $0{\cdot}84$) would apply. These curves, eight of which have been transferred to Fig. 1, cover practically the whole range of conditions likely to be met with where gaugings are wanted. It was inevitable that the standard diagram should not give directly the value of $\frac{v}{\mathrm{V}}$ for any particular case, but should need correction in accordance with the shape factor. This rule will invariably increase the value of $\frac{v}{\mathrm{V}}$ above the standard, so that the range of actual values will be found to extend above $0{\cdot}84$.

As the experimental errors sometimes exceed the percentage of difference between the actual and the standard velocity ratios,

there are naturally some cases where the modification is apparently in the wrong direction, but on the whole it will be found that mean velocities calculated from observed maximum surface velocities in fairly regular channels in the manner here described, will rarely differ by as much as 5 per cent. from the results given by much more elaborate and costly methods of gauging.

It is perhaps doubtful whether it is strictly correct to apply the factor $\left(\frac{4}{3 + \frac{W}{P}}\right)$ over the whole range of conditions shown on the diagram, as there is some evidence that in small rectangular channels, at velocities of less than 2 feet per second, the factor might be

$$\left(\frac{3}{2 + \frac{W}{P}}\right).$$

Another point suggested by these conclusions is that if the hydraulic radius is not a perfect factor in determining the velocity ratio, it might not be perfect in determining the mean velocity. The Author is inclined to think that it is not perfect, but that its imperfection does not affect the mean velocity results to one-third of the extent of the correction required for the velocity ratio, and therefore it would be extremely difficult to trace the effect in ordinary experimental results.

The velocity ratios shown in Fig. 1, in relation to the values of R and v, may be taken to apply approximately to all fairly smooth channels, including earth, sand, cement, brick, sawn timber and ashlar masonry. Rubble masonry and channels with large pebbles or rough stone beds will show materially lower velocity ratios, and should be preferably avoided as gauging stations, but to give an approximate rule it may be stated broadly that the percentage of reduction in velocity ratio in a rough channel, as compared with a smooth one, will be about one-third of the percentage difference in the mean velocities for the given slope and hydraulic radius. For example, if a smooth channel gave $v = 4$ feet per second, and $\frac{v}{V} = 0 \cdot 80$, and a rougher channel of the same size and shape gave $v = 3$ feet per second with the same slope, the value of $\frac{v}{V}$ in the rougher channel will be :—

$$0 \cdot 80 \times \left\{1 - \frac{1}{3}\left(\frac{4 - 3}{4}\right)\right\} = 0 \cdot 733.$$

This rule is given with some diffidence as the data are not so abundant as could be wished, but in order to enable it to be applied when there are no slope observations available for comparing the discharge capacity with Fig. 1, the relative discharges may be calculated from the formulas given in Table I.

A special difficulty might be expected in the cases of circular or oval sewers flowing nearly full, owing to a doubt as to whether the expression $\left(\dfrac{4}{3 + \dfrac{W}{P}}\right)$ fairly represents the effect of shape under the extreme case of the water-level approaching the crown of the sewer. Under these conditions, indeed, the laws of flow have never been tested by any published set of experiments, and no engineer relies upon the theory that the maximum discharge of a sewer occurs when it is not quite full.

Following the rule referred to in the case of circular sewers, it would appear that when flowing half full the velocity ratio would be 10 per cent. above the standard $\frac{v}{V}$, and when flowing nearly full, about 30 per cent. above (the limit being 33 per cent. at full depth). There is nothing improbable about these figures, as the surface velocity when flowing nearly full approximates to the value of the bottom velocity when flowing half full, and therefore would naturally be less than the mean velocity; and it is quite probable that the actual value of $\frac{v}{V}$ in a sewer running nearly full would be greater than unity. It is satisfactory to find that the best recent researches on the distribution of velocity in large pipes, as shown by Pitot tube experiments,[1] are quite consistent with the rule in question applied to sewers running nearly full.

The Author believes that, when further data are available, his ripple-gauge method of measuring surface velocities, coupled with a velocity ratio table, can be made very accurate, and of value to those who have to make gaugings in sewers.

Whilst advocating the velocity ratio method of gauging as sufficiently accurate for the ordinary daily records of discharges and for economically obtaining hydrological data which would not otherwise be recorded, the Author deprecates the exclusive adoption of

[1] Williams, Hubbell and Fenkell, "Experiments at Detroit, Mich., on the Effect of Curvature upon the Flow of Water in Pipes." Transactions Am. Soc. C.E., vol. xlvii (1902), p. 1.

any one method of gauging without checking it by other methods when convenient opportunities occur, so that data may accumulate to verify and, if necessary, to correct the data now put forward.

The relation between mean velocity, hydraulic radius and erosive or scouring power.—Fig. 2, Plate 5, shows the relation between depth, mean velocity and scouring power. The vertical scale gives the logarithms of the depths or hydraulic radii, and the horizontal scale gives the logarithms of the mean velocities. The curves divide the diagram into seven zones, according to the scouring power, as follows:—

I. Mud and silt not moved.
II. Fine silt carried.
III. Heavy silt and fine sand carried.
IV. Coarse sand moved.
V. Small pebbles (size of peas) and gravel moved.
VI. Large pebbles (size of hens' eggs) and coarse gravel moved.
VII. Large stones moved.

It must not be assumed that the boundaries of these zones are quite the hard and fast lines which the exigencies of a diagram require. There is a certain amount of shading-off to be done mentally, to account for graduations in the sizes and specific gravities of the materials.

All scouring action depends mainly upon the degree of turbulence of the flowing water: the boundary lines may be regarded therefore as contours of equivalent degrees of turbulence at various depths and mean velocities, and their directions are sufficiently accurate for most practical purposes. The notes on the diagram are self-explanatory, and represent some of the more interesting data utilized in locating the curves. The upper parts of the curves have been located from a large quantity of data derived from the nature of river-beds in relation to their depths and velocities and also in some cases from currents in estuaries and in the open sea.

Data for the lower parts of the curves were found in irrigation works records, notably in India. In this connection the work of Mr. R. G. Kennedy should be mentioned.[1] He aimed at defining the relation which should subsist between the depth and mean velocity in Indian irrigation canals in order to avoid any appreciable silting, and arrived at the formula:—

$$v = 0 \cdot 84\, d^{0 \cdot 64}$$

[1] Minutes of Proceedings Inst. C.E., vol. cxix, p. 281.

in which v is the mean velocity in feet per second, and d is the depth in feet. This formula was based upon the results of thirty cases in which the depth varied between 2 feet and 7 feet, and these limits are indicated on the diagram.

The region of the experiments of Dubuat and Login are shown by horizontal lines, and the lower part of the diagram shows the location of Mr. J. A. Seddon's interesting experiments with sand in troughs.[1]

To obtain data for very small depths the Author made a number of observations on streams flowing across sandy beaches at low-tide, some of them being mere films of water gliding over a slope of sand.

These observations fully justify the bend in the boundary curve between zones I and II, which corresponds closely with the critical velocity regions found in small pipe experiments, and shows that with depths below 1½ inch the critical velocity increases as the depth decreases, whilst in depths greater than 1½ inch the reverse is the case.

To obtain other evidence as to the curvature of the boundary lines on each side of zone II, with depths of from 1 inch to 3 feet, the Author has observed the velocities and depths in about sixty natural channels at points where the bed was changing from silt to sand, or sand to gravel, that is to say, in channels of diminishing sectional area, and also in places where the reverse conditions existed, and in some cases, where the flow was controllable, observations were made at varying depths. The weight of the evidence thus obtained is in accordance with the curves on the diagram (Fig. 2).

As regards the outward bend of the curves at the boundaries of zone VI (dotted on the diagram) no direct experiments were made. A little consideration will show that they must end practically horizontally, for it is obvious that there is a minimum depth of stream that can move a stone of a certain size. Extremely high velocities may be left out of consideration because they will involve slopes, down which the stones would roll without the assistance of a stream of water.

Several instances of sand-waves in the bed of the channel are noted in zone III, the waves varying between 1 inch high in 2½ inches depth of water, and 5 feet and 15 feet high in the lower Mississippi. The pebble waves on the bed of the Garonne, observed by Baumgarten, are noted in zone V. Two instances of standing surface-waves in flowing water are also given. It has been said that

[1] Journal of the Association of Engineering Societies, vol. 5 (1886), p. 127.

these standing waves are caused by irregularities or sand waves in the bed, but no such cause existed in these cases, and where sand-waves existed the Author has not found standing surface waves.

In a few cases the silt ratios are mentioned, but no general deduction can be made from these figures, because they seldom depend upon the hydraulic conditions at the site of observation in rivers, but are influenced by circumstances existing for great distances up stream; but the case of the sand ratio in sand-pump dredger pipes reaching 1 in 6 at velocities of 15 to 20 feet per second is worthy of note.

In the lower reaches of the Mississippi the silt ratio is not always highest when the velocity is highest, but depends more directly upon the silt ratio in the Missouri; thus showing that the rate of settlement of silt in a river of the tortuous character of the Mississippi is very slow, and that a tributary containing a large quantity of silt may obscure any definite study of the natural silt ratio in a main river over hundreds of miles. In like manner it would seem that the Mississippi Commission's observations on "scour and fill" in relation to depth and mean velocity are obscured by the question of the varying degree of turbulence in the water arriving at the experimental site, which depends not only on the hydraulic elements at that site, but also on the conditions of curvature, depth and velocity for a considerable distance up stream. Whether any "scour" or "fill" takes place in any particular reach also depends upon whether the water is already more or less loaded with a ratio of silt appertaining to the hydraulic conditions existing in that reach.

It has been held by many engineers that scouring power depends solely on the bottom velocity in a stream, but in the Author's opinion that view is inaccurate. Turbulence of motion is the real determining factor, and that depends upon the depth and the mean velocity as indicated in the diagram (Fig. 2). Substituting bottom velocity for mean velocity only accentuates the differences due to depth, as will be seen by studying vertical velocity curves. Below the critical velocity the movement is of the stream-line order, and in an open channel the vertical velocity curves theoretically should be straight lines. Révy's observations on the La Plata, which showed them to be so, have been criticised because they differed from other observations on large rivers at higher velocities by showing the maximum velocity at the surface. The critical-point phenomenon was not then recognized in regard to large channels. Révy found that the bottom velocity increased faster than the surface velocity in the La Plata. Various observers have found that on the lower Mississippi the vertical velocity curves are very upright curves, showing the bottom

velocity to be nearer the mean velocity than Révy observed, and placing the maximum velocity well below the surface. Such curves are found in the neighbourhood of the critical point. At still higher velocities the curves develop a more parabolic shape, with the maximum velocity still well below the surface, and the bottom velocity not increasing in as high a ratio as the mean velocity; a reversal of what occurs below the critical region.

The boundaries of the zones shown in Fig. 2 have been plotted on to Fig. 1, and the very close agreement between the boundary line of zones I and II with the location of the lower boundary of the critical region in Fig. 1, is remarkable and satisfactory, as indicating that the commencement of scour coincides with the change in the mode of motion of the water.

In conclusion, it may be desirable to call attention to some engineering problems upon which the foregoing investigation has a practical bearing. With the rapid growth in the size of ships, the increase in depth of the entrances to harbours and river estuaries is a work upon which far larger sums of money must be spent in the future than in the past. The use of the suction dredger has enabled engineers to accomplish rapidly what training works could effect only after a considerable time, if at all; but it is probable that a judicious combination of training works and suction-dredging may be best in many cases, and in order to secure the most economical results the hydraulics of large channels should be studied with more care than has been customary hitherto. In irrigation works there is room for improvement in the science of dealing with silt, in order to convey it to the places where it is useful, and to avoid deposit or scour in the canals. This problem also is becoming more important owing to the larger scale upon which such works are now planned, and the correct gauging of the water by rapid and economical methods is needed in these works as much as in other water-supply undertakings. The Author hopes that the present Paper will be of practical value in facilitating the solution of these problems.

The Paper is accompanied by two tracings from which Plate 5 has been prepared, and by the following Appendix.

APPENDIX.

TABLE I.—RATIO OF $\frac{\text{Mean Velocity}}{\text{Maximum Surface Velocity}}\left(=\frac{v}{V}\right)$ IN SMOOTH CHANNELS.

Hydraulic Radius in Feet.	Standard Values of $\frac{v}{V}$ for Channels of Unlimited Width.							
	0·70	0·72	0·74	0·76	0·78	0·80	0·82	0·84
	Mean Velocities at which the above Ratios Occur. Feet per Second.							
0·10	0·10	0·92	3·1					
0·20	0·13	0·71	2·0	5·80				
0·50	0·17	0·53	1·18	3·00	7·6	18·5		
0·75	0·18	0·50	0·99	2·24	5·4	12·9		
1·00	0·20	0·53	0·90	1·89	4·3	9·9	22·5	
1·50	0·22	0·58	0·84	1·57	3·12	6·9	15·4	
2·0	0·25	0·60	0·87	1·43	2·60	5·5	11·8	26·0
3·0	0·28	0·62	0·98	1·34	2·19	4·05	8·2	17·2
4·0	0·30	0·66	1·10	1·38	2·02	3·45	6·5	13·0
5·0	0·33	0·71	1·19	1·47	1·98	3·15	5·65	10·8
6·0	0·37	0·75	1·28	1·58	1·99	3·05	5·15	9·2
8·0	0·43	0·82	1·42	1·78	2·12	2·88	4·50	7·55
10·0	0·47	0·88	1·54	1·98	2·28	2·89	4·24	6·70
20·0	0·61	1·13	1·95	2·78	3·15	3·56	4·30	5·60
40·0	0·82	1·45	2·46	3·82	4·49	4·96	5·55	6·35
60·0	0·97	1·70	2·84	4·50	5·55	6·15	6·75	7·62
100·0	1·20	2·00	3·35	5·45	7·15	7·97	8·70	9·70

TABLE II.—FORMULA AND COEFFICIENTS FOR THE FLOW OF WATER IN PIPES AND OPEN CHANNELS.

$$v = \frac{R^{x+y\sqrt{\frac{z-R}{R}}}}{C\,S^{\frac{1}{n}}}.$$

Where v = mean velocity in feet per second,

R = hydraulic radius in feet,

S = cosecant of slope = $\frac{\text{length}}{\text{fall}}$.

When R is greater than z the term $y\sqrt{\frac{z-R}{R}}$ is omitted from the formula.

TABLE OF CONSTANTS AND COEFFICIENTS.

Description of Surface.	n	C	x	y	z
Plain wrought iron	1·80	0·004787	0·65	0·018	0·07
Riveted sheet iron	1·825	0·005674	0·677	?	?
New cast iron [1] (A)	1·85	0·005347	0·67	?	?
,, ,, ,, (B)	2·00	0·006752	0·63	?	?
Lead	1·75	0·005224	0·62	?	?
Pure cement rendering . . .	1·95	0·006429	0·61	?	?
Brickwork (in good condition) .	2·00	0·007746	0·61	?	?
(rather rough) . . .	2·00	0·008845	0·625	0·01224	0·50
Unplaned plank	2·00	0·008451	0·615	0·03349	0·50
Hammer-dressed masonry . .	2·00	0·011166	0·66	0·07825	1·00
Rough stony earth	2·00	0·021444	0·78	?	?
Smooth earth or silt	(See Fig. 1 and text of Paper.)				

[1] In the case of cast iron the set of coefficients which gives the lowest discharge should be used. The set (A) applies below 6 feet per second and (B) above that velocity.

TABLE III.—RESULTS OF EXPERIMENTS MADE BY THE AUTHOR BELOW THE CRITICAL VELOCITY.

R	S	v	Notes.
Feet.	1 in	Feet per Second.	
7·05	332,000	0·027	*River Thames.*
7·12	66,400	0·180	Discharge observed by varying number of paddles open at weir.
7·33	46,110	0·213	
7·45	52,695	0·197	
7·50	43,680	0·279	
7·72	29,120	0·421	
8·15	22,132	0·665	
2·475	1,831,000(?)	0·0094	*River Kennet.*
2·37	610,400	0·0189	Discharge observed by varying opening of mill-sluice.
2·33	366,240	0·0336	
2·356	261,600	0·0566	
2·32	130,800	0·110	
2·32	122,080	0·171	
2·356	70,430	0·289	
2·60	36,624	0·639	
0·3125	133,510	0·0241	15-inch cast-iron pipe 38,855 feet long, rather incrusted. Discharge by water-meter.
,,	17,042	0·445	
,,	3,758	0·772	
,,	1,146	1·380	
,,	1,038	1·488	

(*Paper No. 3690.*)

"Floods in Southern India."

By JOSEPH MELVILLE LACEY, Assoc. M. Inst. C.E.

THE floods in the Penner river due to the cyclone of October, 1874, have been already described.[1] The present Paper deals with the floods in the same river due to the cyclone of November, 1903.

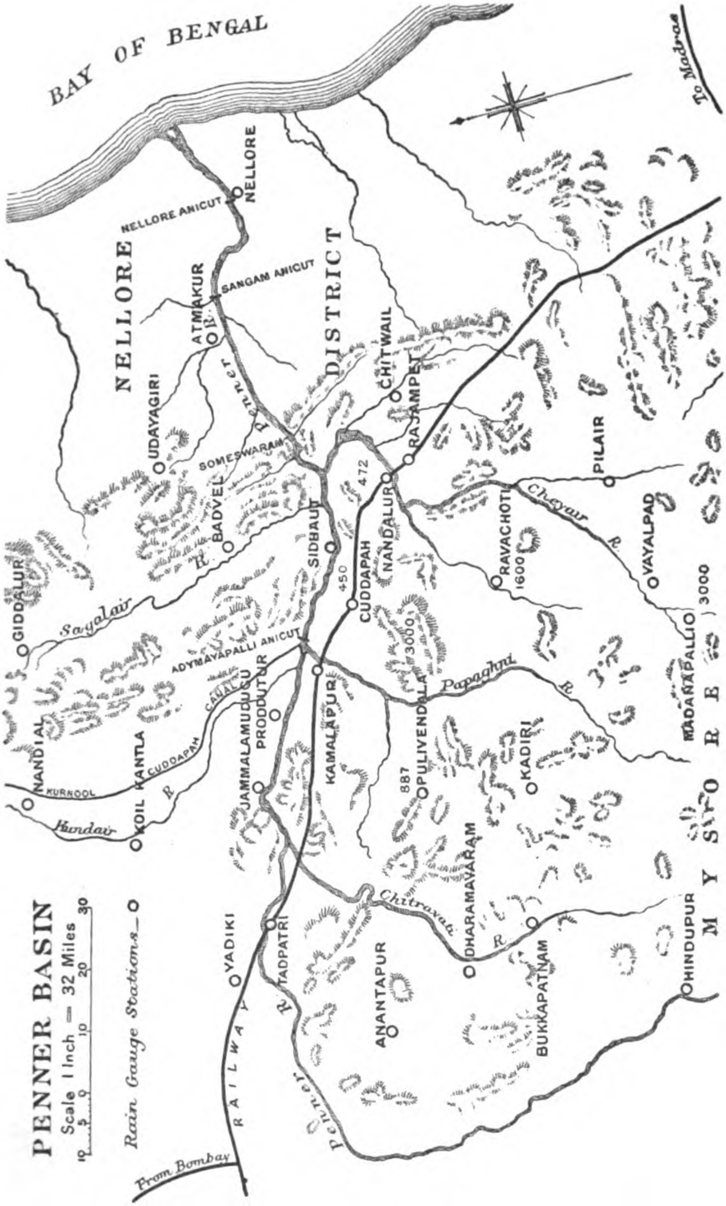
The Penner and three of its main tributaries, the Cheyair, the Papaghni and the Chitravati, take their rise in what is called the Mysore plateau, which is at an altitude of between 2,500 and 3,000 feet above sea-level. These rivers fall to the level of the Cuddapah basin, about 500 to 600 feet above sea-level, where they are joined by the Kundair, another large tributary which drains the northern portion of the basin. The main river, after traversing the Cuddapah basin, cuts through the Velligondas, or eastern ghats, by a clean-cut pass or gorge at Someswaram, and flows in a direct line to the sea through the Nellore low country. *Fig. 1* is a map of the basin of the Penner, showing the position of rain-gauge stations and the heights above mean sea-level. In the Mysore plateau, the river traverses a rugged upland country with rounded and angular groups of hills, but on the borders of the Cuddapah basin, these are replaced by long lines of scarped precipitous cliffs, behind which lie gentle and uniform plains. The upper part of these ranges of hills is principally bare rock, and there are no large forests or thick undergrowth to act as flood-moderators. Freshets in the Penner are consequently of short duration, and for the greater part of the year the river is only a small stream meandering through a waste of sand, but it is capable of being transformed into a mighty flood.

The great floods in the Penner have all been due to cyclonic rainfall. The regular south-west and north-east monsoons,

[1] E. W. Stoney, "Extraordinary Floods in Southern India: their Causes and Destructive Effects on Railway Works." Minutes of Proceedings Inst. C.E., vol. cxxxiv, p. 66.

although occasioning heavy rain in particular localities, do not cause

Fig. 1.



that widespread heavy rain which accompanies a cyclone striking

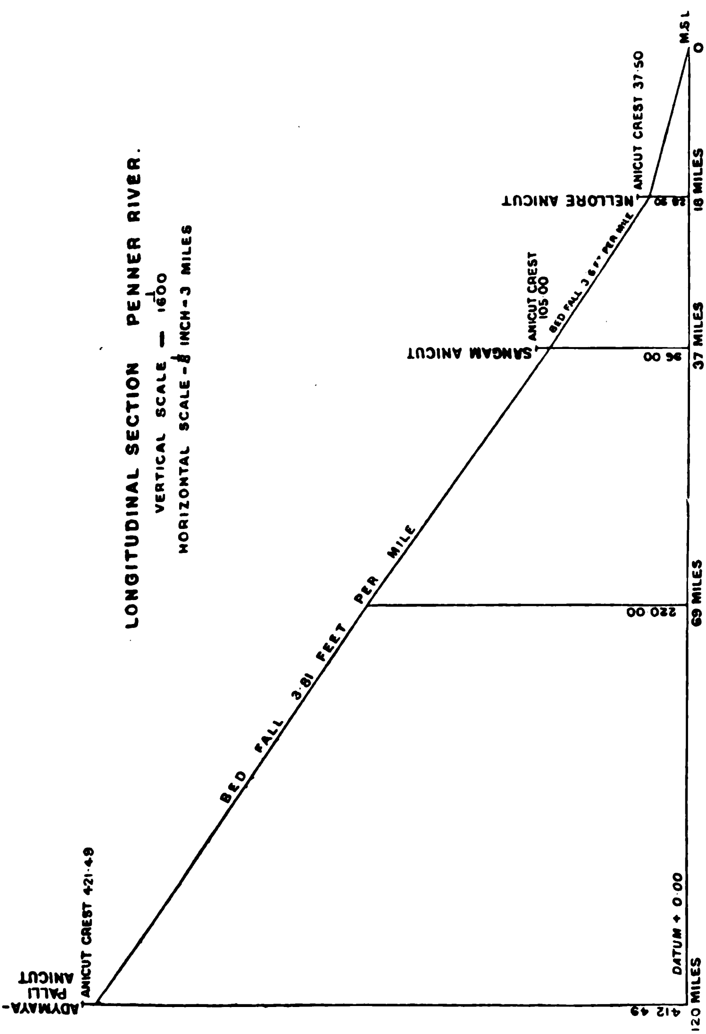
the Madras coast. The cyclone of November, 1903, is described by the Meteorological Reporter to the Government of India as forming to the west of the Little Andaman Islands on the 3rd November, and travelling westwards on the 4th and 5th of that month. It struck the Madras coast near Madras on the 6th, and then diminished considerably in intensity. Being unable to surmount the eastern ghats, the residual disturbance re-curved to the north-east, and on the 7th, it lay to the north-east of Madras, where it filled up slowly during the next two days. "The storm, although of slight intensity, occasioned very heavy rain in the south of India, resulting in extensive floods." In the Appendix is given the rainfall at the various stations in the basin of the Penner, from which it will be seen the rainfall traced from east to west on the 6th is as follows: Pilair, 7·02 inches; Rayachoti, 9·77 inches; Kadiri, 6·79 inches; Bukkapatnam, 4·30 inches; Dharmavaram, 2·64 inches; Anantapur, 2·5 inches, and so on. The cyclone spent itself at Bellary. A further fall of 5·89 inches appears to have occurred at Rayachoti on the 7th, due probably to the scarped ridge which runs from 2,900 feet to 3,000 feet above sea-level, and which forms the north-western boundary of the Taluk, intercepting the rain-bearing clouds. Pulivendala, which is below this ridge, received only 3·20 inches on the 6th and 0·90 inch on the 7th. Tracing the rainfall from south to north it is found to be: Rajampet, 7·94 inches; Chitwail, 6·26 inches; Sidhaut, 3·80 inches; Badvel, 2·86 inches; and Giddalur, 0·75 inch, where the rain spent itself. There appears to have been a lull and a second phase of heavy rainfall on the 10th and 11th, confined more or less to the coast. From the above description it will be seen that the heavy rainfall was widespread over the whole basin.

It should be noted also that the rain-gauges are read at 8 A.M. every morning, and the amount gauged denotes the rainfall during the preceding 24 hours. Thus the rainfall given in the Table for the 6th is the rainfall between 8 A.M. of the 5th and 8 A.M. of the 6th.

There are three important anicuts or weirs across the Penner, namely, at Nellore, Sangam and Adymayapalli. Their positions are shown in *Fig. 1*, and *Fig. 2* gives a section of the river with the level of the crests of these anicuts, from which it will be seen that the Penner has a bed-fall of 3·81 feet per mile from the Adymayapalli anicut to the Sangam anicut, and of 3·6 feet per mile from the Sangam to the Nellore anicut. The river-bed here is compact coarse sand. *Figs. 3* and *4* show the flood-levels over the anicuts, for the period under discussion, obtained from the respective water-registers. The upper line shows the depth over crest up

stream of the anicut, and the lower line shows the depth over crest down stream, when the anicut becomes a submerged weir. It will be seen from these figures that two distinct high floods occurred,

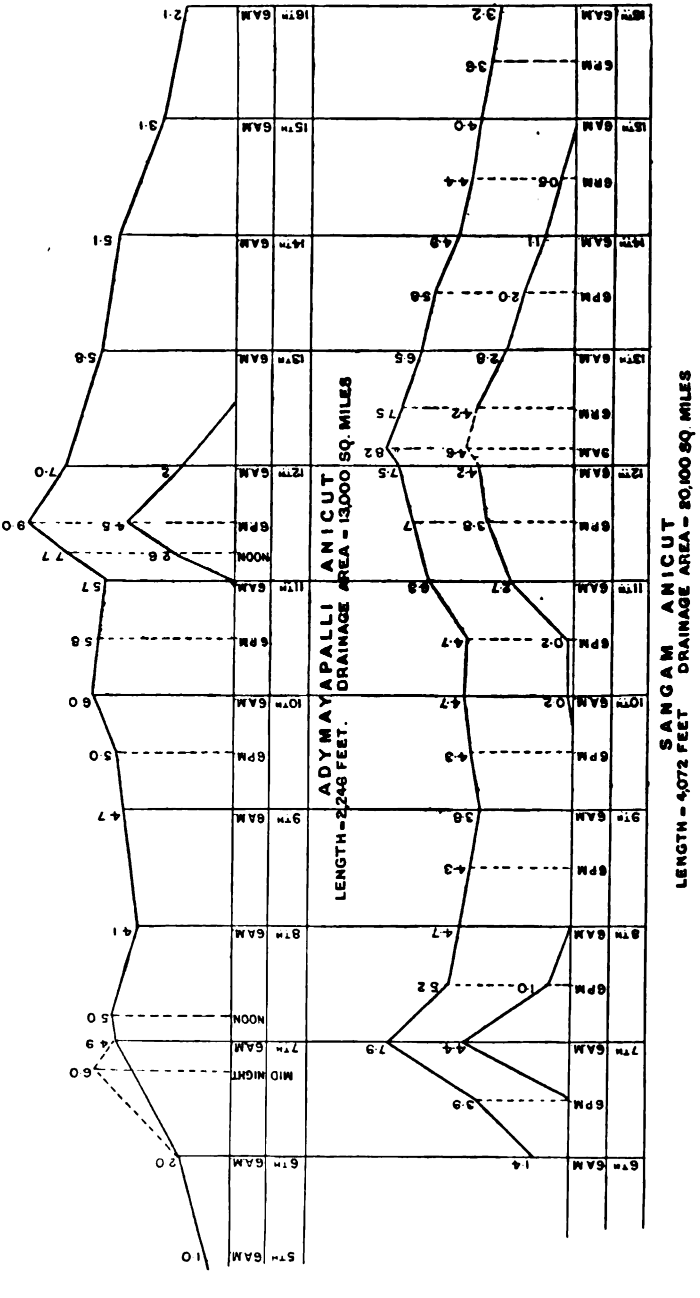
Fig. 2.



one on the morning of the 7th and the other between the 11th and 12th.

The mean velocity of the maximum flood on the 11th from the Adymayapalli anicut to the Sangam anicut was 5·53 miles per hour,

Fig. 3.


ADYMAYAPALLI ANICUT
LENGTH - 2,248 FEET. DRAINAGE AREA - 13,000 SQ. MILES
SANGAM ANICUT
LENGTH - 4,072 FEET DRAINAGE AREA - 20,100 SQ. MILES

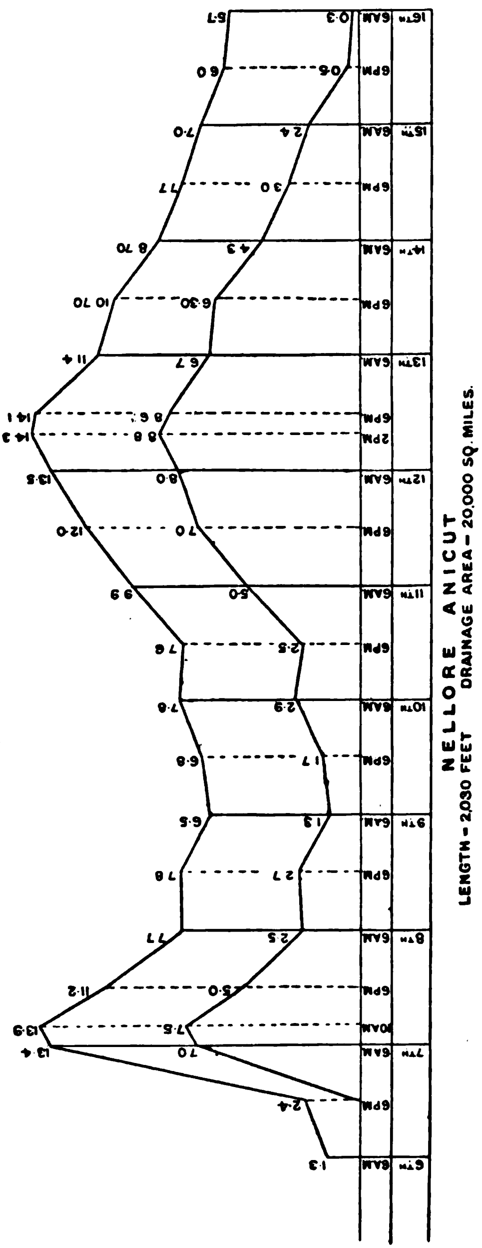

NELLORE ANICUT
LENGTH - 2,030 FEET
DRAINAGE AREA - 20,000 SQ. MILES.

Fig. 4.

and from the Sangam anicut to the Nellore anicut, on the 12th, nearly 4 miles per hour, with a full river and consequently a flat hydraulic gradient. It is interesting to note that on the 7th, when the river was empty and the hydraulic gradient steeper, the velocity of the flood from the Sangam to the Nellore anicut was nearly 5 miles per hour. The maximum flood, which was probably due to rainfall registered at 8 A.M. on the 6th, passed down the Cheyair, reaching the railway-bridge near Nandalur at 4 P.M. on the 6th.

It is to be observed that a greater flood occurred on the 11th and 12th than on the 6th, although the average rainfall on the 6th was 3·56 inches over the whole basin, while the average for the 11th was only 1·79 inch. The probable reason is that on the 6th and 7th the river was empty and a portion of the flood-water consequently went to fill it, the river acting as its own flood-moderator. The rainfall of the 11th coming on an already swollen river considerably augmented its flood. It is also very probable that the first flood at the Sangam anicut at 6 A.M. on the 7th was due to the floods down the Cheyair and to the minor drainages between the Sangam and Adymayapalli anicuts. The above shows how difficult it is to devise a formula for estimating the maximum floods that may be expected in rivers having such extensive basins.

The Author has endeavoured to arrive at some practical results from the observations here recorded. The maximum flood in the Cheyair passed the bridge at 4 P.M. on the 6th, and reached the Sangam anicut at 6 A.M. on the 7th, which is equivalent to a velocity of 6·75 feet per second: if allowance is made for the sharp bend below the bridge and the increase of area in the main river, a mean velocity through the bridge of 7 feet per second may be taken as correct, and this corresponds to a discharge of 172,200 cusecs.[1] The rainfall on the basin as registered at 8 A.M. on the 6th gives:—

	Inches.	
Rayachoti	9·77	Mean = 7·56 inches in 24 hours.
Madanapalli	6·05	
Vayalpad	7·40	
Pilair	7·02	

and since the drainage-area is 2,100 square miles, the fall is equal to 428,650 cusecs, and the discharge is 40 per cent. of the rate of fall. As a comparison, Mr. Stoney's calculations in the Paper already referred to for the flood of 1874 gives the discharge as 39·5 per cent. of the rate of fall.

The mean intensity of rainfall, however, was probably higher than

[1] I.e., cubic feet per second.

7·56 inches, say 8¼ inches, and this would give a discharge of about 30 per cent. In the case of the Adymayapalli anicut, the difficulty arises that the flood of the 11th was not entirely due to the rainfall of the preceding 24 hours. It may be assumed, however, with some degree of accuracy, that the flood of midnight of the 6th was due to the heavy rainfall on the basin as gauged at 8 A.M. on the 6th. Thus:—

Length of anicut	2,246 feet.
Discharge	176,800 cusecs.
Drainage area	13,300 square miles.
Mean rainfall over basin	2·27 inches.
Rate of fall	815,200 cusecs.
Discharge	21·7 per cent.

As no water came down the Kundair, which drains 3,300 square miles, this area must be eliminated: the mean fall is then 3 inches on 10,000 square miles. Hence:—

Rate of fall	810,000 cusecs.
Discharge	22 per cent., nearly.

In the case of the Sangam anicut, the flood of 6 A.M. of the 7th gives the following:—

Length of anicut	4,072 feet.
Discharge	398,241, say 400,000 cusecs.
Drainage area	20,100 square miles.
Mean rainfall over basin	3·56 inches.
Rate of fall	1,832,000 cusecs.
Discharge	20·5 per cent.

Taking the difference of discharge between the Sangam and Adymayapalli anicuts for the 11th and 12th, which equals 160,000 cusecs:—

Difference in drainage area	6,800 square miles.
Mean rainfall on this area at 8 A.M. on the 11th	2·80 inches in 24 hours.
Rate of fall	514,080 cusecs.
Discharge	31 per cent.

The discharge for 7,000 square miles, therefore, may be taken as 30 per cent.

For a drainage-area of 260 square miles the Author obtained a discharge of about 60 per cent., and for a small catchment of 20 square miles, a discharge of 90 per cent. The 20-square-mile catchment, however, was very favourable; it was almost semi-circular in shape and was situated in a rugged country. Using the data obtained in these observations and in connection with the Sangam and Ady-

mayapalli anicuts, calculations similar to those recorded above gave the following results:—

PENNER BASIN.

Catchment-Area.	Discharge that may be expected from Cyclonic Rainfall.	Catchment-Area.	Discharge that may be expected from Cyclonic Rainfall.
Square Miles.	Per Cent.	Square Miles.	Per Cent.
20	90	7,000	30
260	60	10,000	22
2,100	40	20,000	20

No calculations were made in regard to the Nellore anicut, as the river at that point is contracted to 2,030 feet, and a standing wave forms below the anicut during high floods. Although no great degree of accuracy is claimed for these results, which are furthermore subject to a correction to allow for the mean intensity of rainfall being greater than the records, they may be taken as a fair guide in estimating the maximum flood-discharges that may be expected from any area in the Penner basin.

The Paper is accompanied by three tracings, from which the Figures in the text have been prepared, and three weather charts.

[APPENDIX.

APPENDIX.

RAINFALL STATIONS, PENNER RIVER, FOR NOVEMBER 1903.

Name of Station.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.	12th.	13th.
	Nellore District.										
Atmakur	..	0·85	1·30	3·65	0·57	1·64	0·27	1·00	3·65	1·36	0·07
Udayagiri	..	0·50	0·30	2·90	1·62	0·31	0·32	2·05	2·52	2·28	2·27
Nellore	0·33	1·90	3·79	0·34	0·93	1·62	0·61	5·88	2·58	0·35	..
Mean . . .	0·11	1·08	1·80	2·30	1·04	1·19	0·40	2·98	2·92	1·33	0·78
	Basin of the Cheyair. Drainage-Area 3,000 Sq. Miles.										
Chitwail	..	..	0·86	6·26	0·73	0·14	0·27	0·05	3·37	1·48	0·69
Rajampet	..	..	0·53	7·94	0·62	0·12	0·13	0·45	2·50	0·44	0·55
Rayachoti	..	..	0·41	9·77	5·89	0·40	0·22	1·86	1·95	1·30	0·18
Madanapalli	..	..	0·26	6·05	0·51	1·12	2·97	0·99	2·25	0·75	0·90
Vayalpad	..	..	0·41	7·40	0·47	5·03	1·68	0·02	5·32	3·08	0·85
Pilair.	0·40	0·09	0·46	7·02	0·69	0·39	0·94	0·58	1·30	1·00	0·62
Mean . . .	0·07	0·01	0·49	7·41	1·48	1·20	1·04	0·66	2·78	1·34	0·63
	Basin of the Papaghni. Drainage-Area 2,500 Sq. Miles.										
Pulivendala	0·14	..	0·13	3·20	0·90	0·47	0·29	2·45	1·18	0·78	0·15
Kamalapur	..	..	0·47	2·52	1·40	..	..	2·07	1·83	0·70	..
Cuddapah	..	..	0·50	3·34	1·45	0·26	0·15	1·29	2·45	0·74	0·04
Mean . . .	0·05	..	0·37	3·02	1·25	0·24	0·15	1·94	1·82	0·74	0·06
	Basin of Chitravati. Drainage-Area 2,410 Sq. Miles.										
Kadiri	..	0·36	0·18	6·79	1·39	1·88	0·55	0·21	1·70	1·33	0·03
Bukkapatnam . . .	1·46	0·10	0·12	4·30	1·40	0·68	0·65	0·56	1·15	0·35	..
Dharmavaram . . .	0·85	..	0·02	2·64	0·34	1·80	1·19	1·06	0·62	0·38	0·26
Mean . . .	0·77	0·15	0·11	4·58	1·04	1·45	0·80	0·61	1·16	0·69	0·10

Rainfall Stations, Penner River, for November 1903—*continued*.

Name of Station.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.	11th.	12th.	13th.
	Basin of Upper Penner. Drainage-Area 4,062 Sq. Miles.										
Hindupur	0·24	..	0·97	1·82	0·55	1·02	0·30	2·50	0·40	0·35	0·05
Anantapur	0·44	..	..	2·50	0·36	0·85	0·68	0·83	0·66	0·76	0·35
Yadiki	0·45	..	..	1·25	0·31	..	0·68	0·14	0·12	..	..
Tadpatri	0·30	0·79	0·13	1·52	0·82	1·55	..	1·65	0·19	0·33	..
Mean	0·36	0·20	0·28	1·77	0·51	0·86	0·41	1·28	0·34	0·36	0·10
	Basin of Kundair. Drainage-Area 3,300 Sq. Miles.										
Jammalamadugu	0·28	0·21	0·88	1·07	1·81	0·16	0·04	0·26	1·13	0·83	..
Prodattur	..	..	0·86	1·82	1·10	0·18	0·05	2·00	0·41	1·53	..
Nandial	..	0·07	0·19	0·13	0·40	..	0·19	0·41	0·55	0·13	..
Koil Kantla	..	1·06	0·05	0·79	0·57	0·03	0·13	0·20	0·13	..	..
Mean	0·07	0·34	0·50	0·95	0·97	0·09	0·10	0·71	0·55	0·62	..
	Basin of Sagalair. Drainage-Area 1,178 Sq. Miles.										
Sidhaut	0·25	0·01	0·35	3·80	1·05	0·26	0·18	0·72	4·10	0·77	0·05
Sadvel	..	..	1·38	2·86	1·34	0·19	0·13	2·14	2·84	0·61	0·29
Giddalur	..	..	0·29	0·75	0·80	0·09	0·21	0·43	1·57	0·42	0·02
Mean	0·20	0·23	0·57	3·56	1·08	0·78	0·49	1·22	1·79	0·85	0·28
Mean over basin	**0·08**	..	**0·67**	**2·47**	**1·06**	**0·22**	**0·17**	**1·09**	**2·84**	**0·60**	**0·12**

(*Paper No. 3681.*)

"The Heating of Air by Flue-Gases."

By George Edward Tansley, B.Sc., Assoc. M. Inst. C.E.

The Uses of Hot Air.

The use of "hot air" has in recent years become so general, that its economical production is now a matter of great importance in many industries.

It is used extensively as a drying medium, in bleach-works, dye-houses, woollen-factories, paper-mills, laundries, and many less important industries; also as a heating medium in hospitals and other buildings.

One of the more important uses to which it has recently been adapted is in connection with the Wilson gas-producing plant, and another application is that of heating air for combustion purposes in boiler furnaces, instead of using cold air.

Hitherto the principal method of heating the air has been by means of live or exhaust-steam, but the object of this Paper is to describe a series of experiments, which have been conducted by the Author, in order to determine the most suitable form of tube and heater for the utilization of the heat in flue-gases.

Description of Heater.

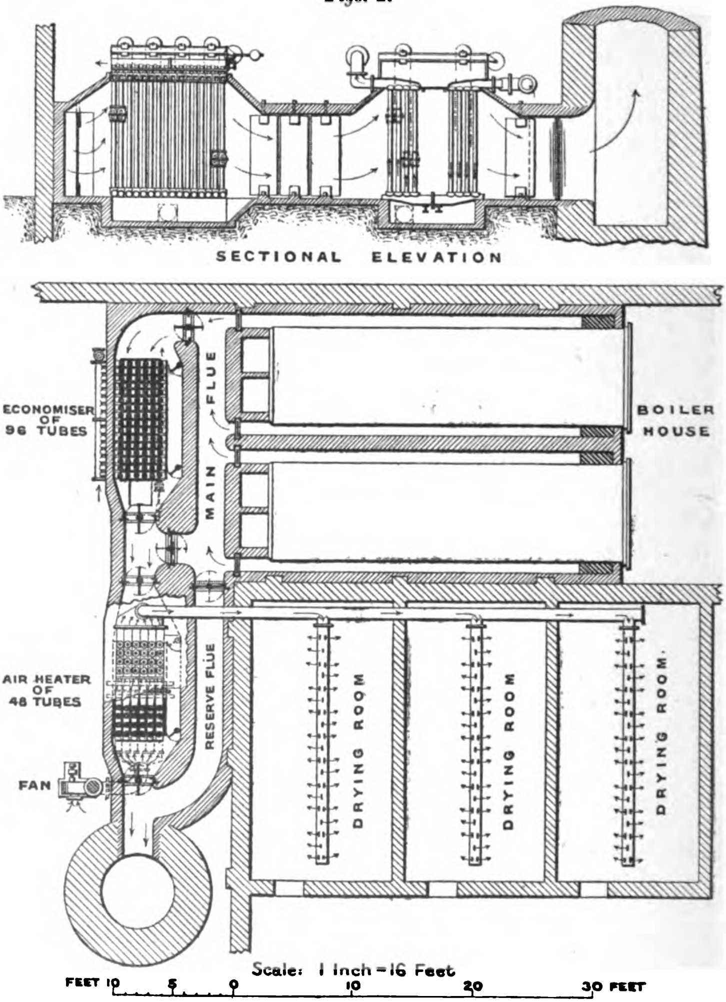
The apparatus shown in *Figs*, *1* represents the latest type of air-heater as manufactured by Messrs. E. Green and Son.

It is specially constructed for supplying a large volume of air for drying purposes, and with certain modifications is designed on the same principle as an economizer, the air passing through the tubes and receiving the heat from the flue-gases in a similar manner. It may be worked separately or in conjunction with an economizer, in the latter case the economizer is usually fixed nearer to the boilers, and so receives the first impact of the flue-gases.

The heater consists of a series of vertical cast-iron tubes about 8½ feet long, the ends of which are pressed into top and bottom boxes

of a sloping design (*Figs. 2*), the top boxes having lids for the purpose of inspection and renewal of the pipes. These top and bottom boxes are so sloped or tapered that the opening of the box is greatest

Figs. 1.



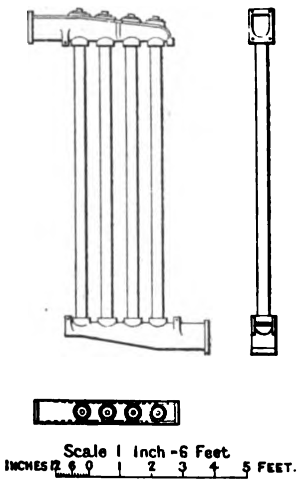
AIR-HEATER AND DRYING ROOMS.

where the largest volumes of air have to pass; also all sharp corners and bends are eliminated as far as possible, the air being thus easily deflected from one portion of the heater to another with a

minimum loss of head. It is therefore kept at a uniform speed and pressure throughout the heater, ensuring a uniformity of temperature of the heated air and a constant pressure on the fan. When the pipes have been pressed up into sections of four or eight pipes wide, as the case may be, these sections are fixed in the main flue across the path of the gases, and are connected up at the top and bottom by air-ducts of ample capacity.

The cold air is forced into the heater by means of a fan, through the air-ducts at the top or bottom as the individual case may require, and it is made to leave the heater at the hot end by an outlet branch-pipe of similar construction to the inlet branch-pipe. From the outlet it is made to pass into air-ducts, covered if possible, through which it passes to its destination.

Figs. 2.



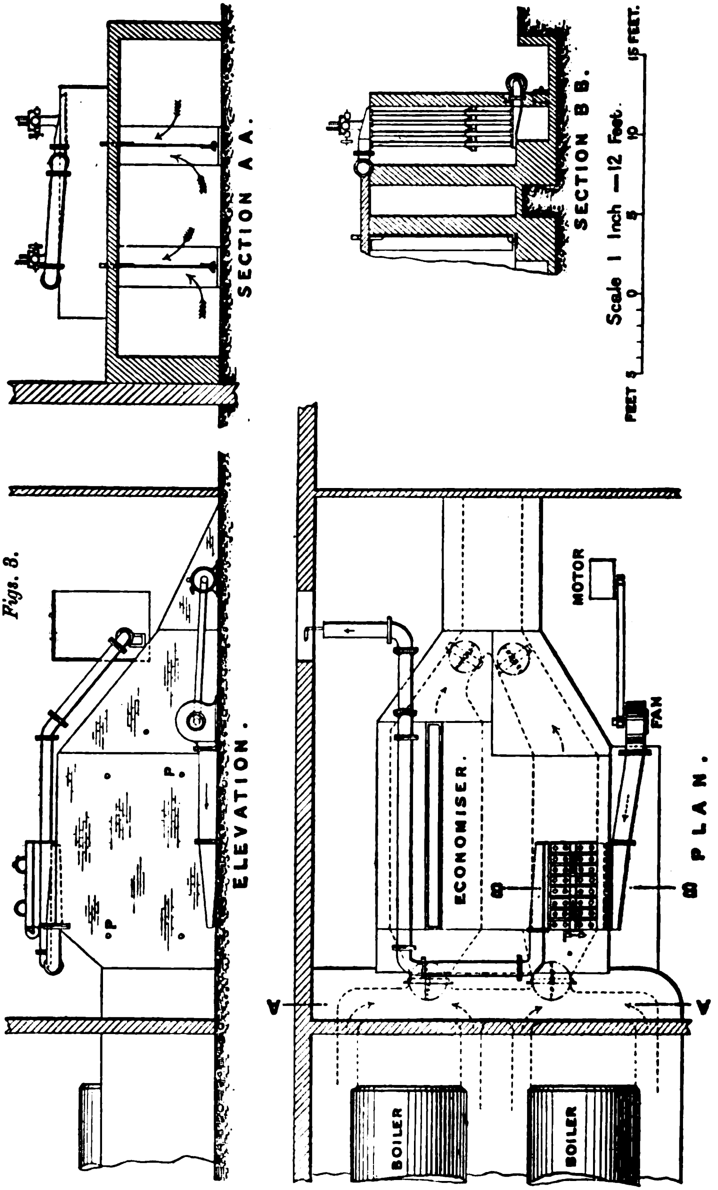
As air is heated by contact only, and as the cast-iron pipes used for this type of air-heater are necessarily of a large bore ($3\frac{7}{8}$ inches) for purposes of rigidity, durability, and convenience of scraping, it becomes necessary to fit something inside each pipe which will force the air into contact with the outer shell as much as possible, in so far as this is practicable with the expenditure of a reasonable amount of power.

Several internal-devices have consequently been experimented upon by the Author, and the comparative usefulness of such devices ascertained both in relation to the plain pipe, and in relation to each other. The Author then made tests upon a heater so arranged that the air was forced down half the pipes and up the other half as shown in *Fig. 5*, in order to justify the claims made for such circulation; and tests have also been carried out upon a heater fitted with thin solid-drawn steel tubes, in order to compare the efficiency of these tubes with that of the comparatively thick cast-iron tubes.

Description of Heat Tests.

Figs. 3 show the arrangement of a thirty-two-pipe air-heater, which was fixed behind two Lancashire boilers for experimental purposes. Part of the gases passed through the air-heater and part through

Figs. 3.



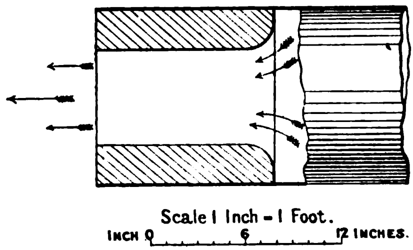
AIR-HEATER USED IN HEAT EXPERIMENTS.

the economizer, the gases meeting again before passing up the chimney.

The air to be heated was forced through the heater by means of a fan, motor-driven, and the speed of the motor was so controlled by a resistance-tank containing a solution of soda, that the volume could be varied from 100 to 2,000 cubic feet per minute. On leaving this heater the air passed through about 34 feet of galvanized sheet-iron piping, 11 inches diameter and uncovered, to a specially shaped orifice, shown in *Fig. 4*.

The volumes of air issuing from the orifice were measured by means of a Pitot tube. A wet- and dry-bulb hygrometer was fixed near the inlet to the fan in order to measure the temperature and humidity of the air entering the heater, and two thermometers were fixed in the outlet-pipe of the heater in order to accurately ascertain the outlet-temperature of the heated air. A thermometer was also fixed near the orifice in order to ascertain the temperature for the Pitot tube observations, and to find what fall in temperature occurred between the outlet from the heater and the outlet from the orifice.

Fig. 4.

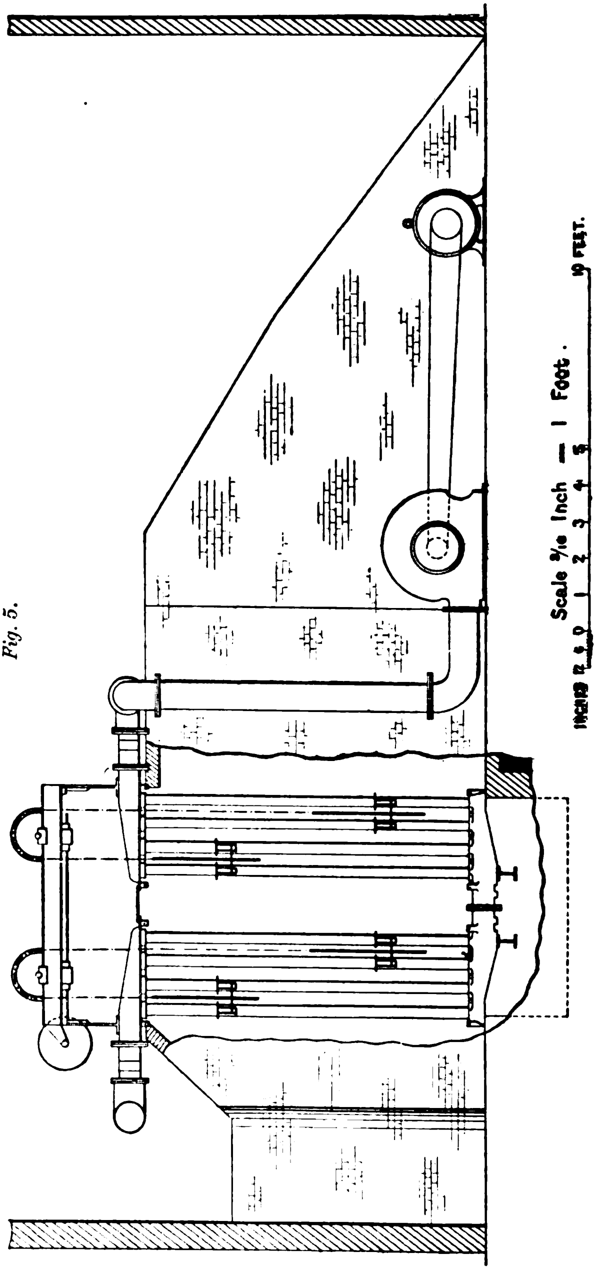


Three mercurial-nitrogen thermometers were fixed at the hot end of the heater, and three at the other end, to determine the mean inlet- and outlet-temperatures of the flue-gases. These six thermometers were necessary, on account of the great difference between the temperature of the flue-gases at the top and that at the bottom of the heater.

A water-gauge was fixed in the air-duct between the fan and the inlet, in order to measure the pressure of the air entering the heater, and a $\frac{1}{4}$-inch tube was fixed in each corner external lid of the heater, opposite each corner-pipe, so that by means of a water-gauge fixed to these tubes it could be ascertained whether each pipe was conveying its proportionate volume of air.

Several series of tests were made, and Appendix I gives a summary of the data obtained and the results derived therefrom, in the series which gave the best results. Two curves were plotted for each series of tests, the first showing the "head in inches water-gauge" which is lost by the air in passing through the heater, and the second showing the "number of British Thermal

Fig. 5.



EXPERIMENTAL AIR-HEATER ARRANGED WITH THE TUBES IN SERIES.

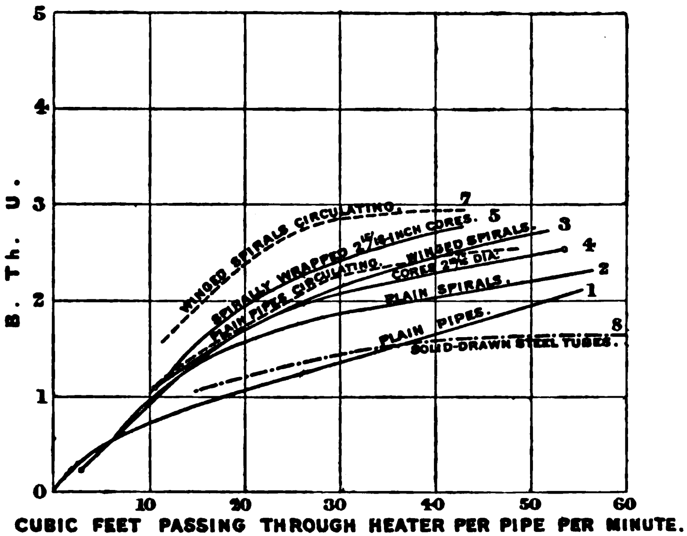
Units absorbed by the air per square foot of heating-surface per hour, per 1° F. difference between the mean temperature of the flue-gases and the mean temperature of the air." The speed of the flue-gases passing the heater was the same in each test, namely, 250 feet per minute.

It will be noticed how high a pressure was required to force the air through the heater. Such pressures are not normal and were due to the following causes:—

(1) The small orifice at the outlet, made small for convenience and accuracy in taking observations.

(2) The great number of bends and curves necessary in this

Fig. 6.



particular heater, so as not to interfere with fixtures in the economizer-house.

A special series of "power" tests have, however, been carried out by the Author, and results (given later) show that a heater can be so arranged that the power required to force the air through it is considerably less than that recorded above.

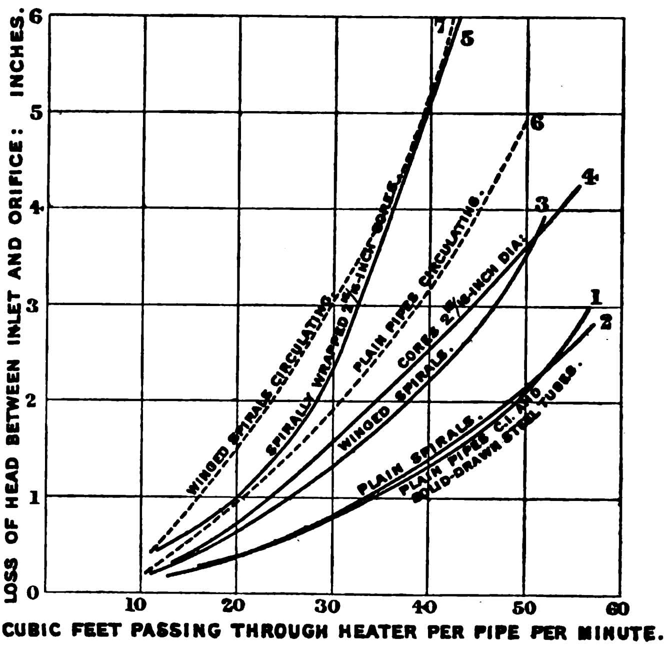
Five series of heat tests were carried out under the following conditions:—

The heater was arranged with thirty-two cast-iron pipes $3\frac{7}{8}$ inches bore and $\frac{3}{8}$ inch thick. The cold air entered the heater at the bottom, passed through all the tubes in parallel, and the heated air issued

from the heater at the top as indicated. From eight to nine separate tests were made in each series, with volumes of air varying from about 200 to 1,800 cubic feet per minute passing through the pipes, except in the fifth series where only five tests were made.

In the first series the pipes were plain; and in the second, the inside of each pipe was fitted with a piece of sheet iron $3\frac{1}{2}$ inches wide and $\frac{1}{16}$ inch thick, twisted in the form of a spiral to give to

Fig. 7.



the air a twisting motion; there was one complete twist in every 3 feet.

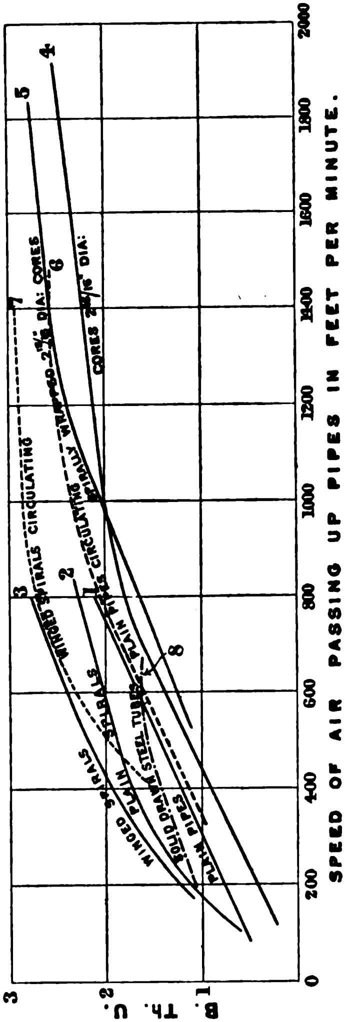
In the third series the plain spirals were replaced by "winged spirals," there being one of these in each pipe. The wings consisted of flat pieces of sheet iron, semi-circular in shape, and fixed at intervals in sawcuts in the two edges of the plain spiral. These wings can be bent to any angle to suit the volume of air passing through the heater with the power at disposal, and have the effect of stirring the air, at the same time forcing it into contact with the surface of the pipe. For this particular series of

tests, the wings were fixed at right angles to the direction of flow of the air.

In the fourth series each pipe was fitted with a sheet-iron core $2\frac{5}{16}$ inches diameter, with conically closed cast-iron ends, which forced the air down the annular space between the core and the pipe, and so brought it into contact with more heating-surface per unit volume of air passing.

In the fifth series, as in the fourth, each pipe was fitted with a core $2\frac{5}{16}$ inches diameter, but the cores were wrapped spirally with asbestos rope, of a diameter equal to the width of the annular space between the core and the pipe. The pitch of the wrapping was 2 feet, so that as the air passed through, it made a little over four journeys round each pipe before passing into the boxes. Five tests only were made with the heater so arranged, because to obtain larger volumes of air than 1,386 cubic feet per minute, the air-pressure would have been altogether impossible for practical working.

Fig. 8.

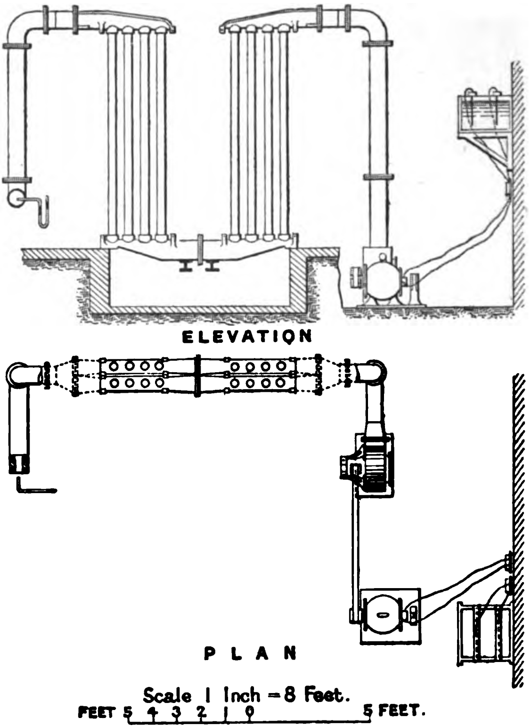


When the above series of tests had been completed, the heater was taken down and re-erected as shown in *Fig. 5*, but with the air passing down sixteen cast-iron pipes, up sixteen, and then out. In the sixth series the pipes and boxes were plain, as in Series 1, and seven tests were made with volumes of air varying from 333 to 1,595 cubic feet per minute.

In the seventh series inside each pipe was fitted a "winged spiral," the wings in this case being bent at an angle of 135° with the direction of flow of the air. Five tests were made with volumes of air varying from 360 to 1,380 cubic feet per minute.

The heater was then again taken down, and the cast-iron pipes $\frac{3}{8}$ inch thick were replaced by thirty-two thin solid-drawn steel

Figs. 9.



tubes 4·25 inches bore and 0·175 inch thick. The air was passed in parallel through the thirty-two tubes, and the volume varied from 460 to 1,888 cubic feet per minute.

As an example, the experimental results obtained in the seventh series of these tests are given in Appendix I, and the important deductions for all the eight series have been plotted as shown in *Figs. 6* to *8*, which are self-explanatory.

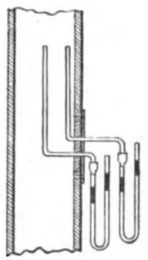
Description of Pressure-Tests.

After these "heat" tests had been completed a further series of tests was made upon a sixteen-tube heater, in order to find out the power required to force different volumes of air through heater tubes of various constructions.

This sixteen-tube heater was not placed in a flue, as it was not considered necessary to again take temperature observations, but it was arranged first as for the first five series of tests with the air going down the sixteen pipes in parallel, and secondly with the air going down eight pipes and then up eight pipes as for the sixth and seventh series. It was fitted with air-ducts having a reasonable number of bends, such as would be the case with a heater working under normal conditions.

The experimental apparatus was the same as in the "heat" tests, with the addition of an ammeter and voltmeter for measuring the power, so as to determine the combined efficiency of the motor, belt, and fan. Water-gauges were also fixed in each outside pipe, in order to make certain that all portions were conveying their proportionate volume of air.

Fig. 10.



A water-gauge (see *Fig. 10*) was also fixed in one pipe in order to ascertain whether the velocity of the air in the centre of each pipe differed from the velocity near the inside surface, but as anticipated by the Author, there was no appreciable difference between the two pressures.

Different volumes of air were forced through the heater under the following conditions, and the results are shown plotted in *Fig. 11*.

Curves Nos. 10 to 13 represent the results obtained with the arrangement shown as in the first five series of tests, the pipe being of cast-iron, $3\frac{7}{8}$ inches bore, $\frac{3}{8}$ inch thick and 8 feet 6 inches between sockets. For curve No. 14 the pipes were only $6\frac{3}{4}$ inches long, and for Nos. 15 and 16 the arrangement was that shown in *Figs. 9*. In curve No. 13 cast-iron Serve tubes were used, the great cost however precludes their use for air-heating purposes.

Curve No. 17 gives the results when the top and bottom branch pipes were bolted together, so that the air passed straight from one branch pipe to the other as shown in *Figs. 13*.

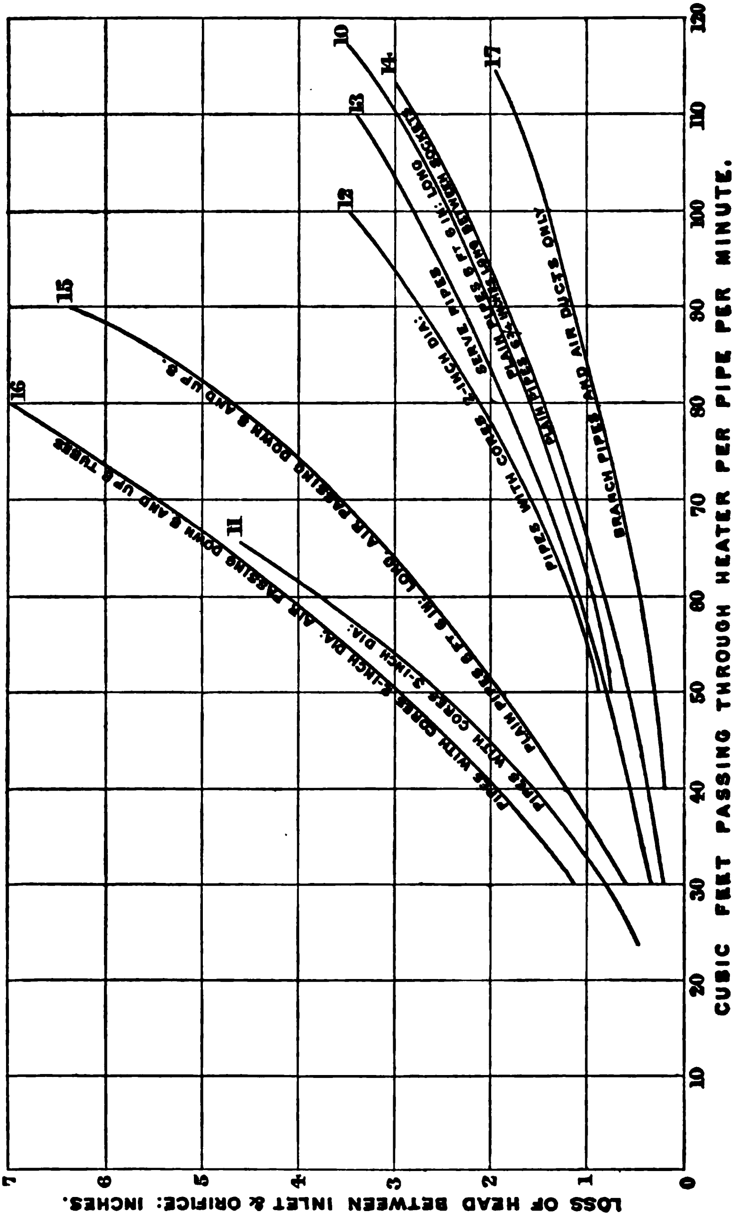
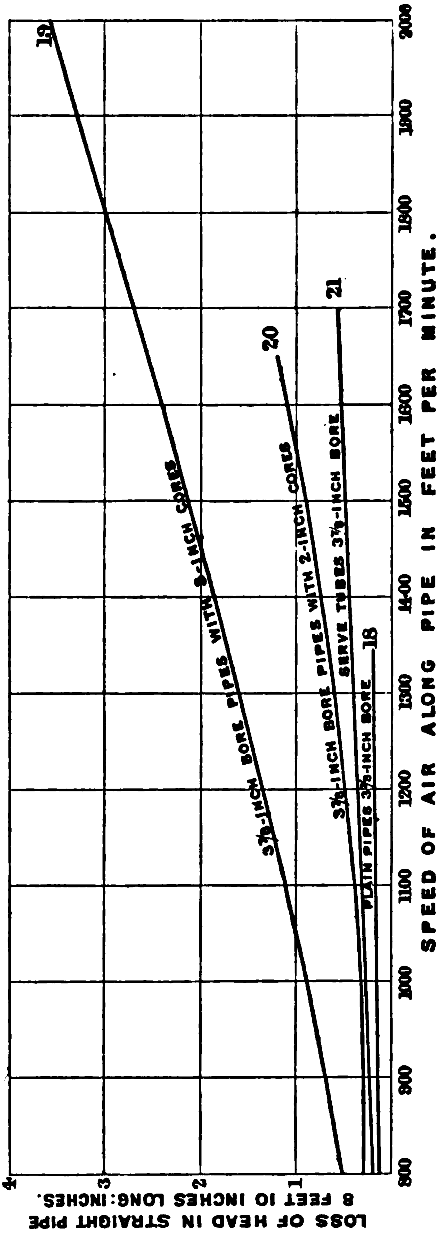

CUBIC FEET PASSING THROUGH HEATER PER PIPE PER MINUTE.
LOSS OF HEAD BETWEEN INLET & ORIFICE: INCHES.
10
14
17
13
12
15
16
11
BRANCH PIPES AND AIR DUCTS ONLY
PIPES WITH CORES 3-INCH DIA:
0
1
2
3
4
5
6
7
10
20
30
40
50
60
70
80
90
100
110
120

Fig. 11.

It will be readily seen that if the difference of the ordinates between two curves, Nos. 10 and 14 be used to plot another curve, this will represent the head lost by the air in passing through sixteen plain cast-iron pipes, 3⅞ inches bore and of a length equal to the 8 feet 6 inches minus the 6¾ inches, namely, 7 feet 11¼ inches. This curve No. 18 has been replotted as a velocity-pressure curve, and gives the head in inches water-gauge, which is lost by air passing at various speeds through these pipes.

It is clear from this curve that the loss of head would be inappreciable for pipes 6¾ inches long; it may therefore be assumed that curve No. 14 represents the head lost by different volumes of air passing through the air-ducts, branch-pipes, and boxes only. Hence if curve No. 14 be subtracted

Fig. 12.



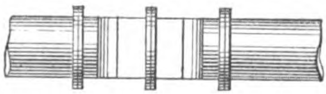
Velocity-Pressure Curves for Straight Pipes only.

from curves Nos. 11 to 13, respectively, curves Nos. 19 to 21 (*Fig. 12*) will be obtained, after replotting as velocity-pressure curves. These will give the head lost by air passing at various speeds along pipes 8 feet 10 inches long, fitted with 3-inch cores, 2-inch cores, and Serve tube pattern respectively (the 8-foot 10-inch length representing the 8 feet 6 inches between sockets plus the 2 inches of pipe in each socket).

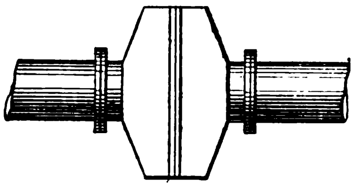
Since plain spirals do not put any extra pressure on the fan, they were not included in this series of experiments, and winged spirals were also omitted because the pressure depends so much upon the angle at which the wings are set.

It will be noticed that curves Nos. 18 to 21 are almost parabolic in form, bearing out the theory that the loss of head due to friction varies as the square of the velocity of the air.

Figs. 13.



ELEVATION



PLAN.

Scale 1 Inch = 3 Feet.

INS. 12 6 0 1 2 3 FEET

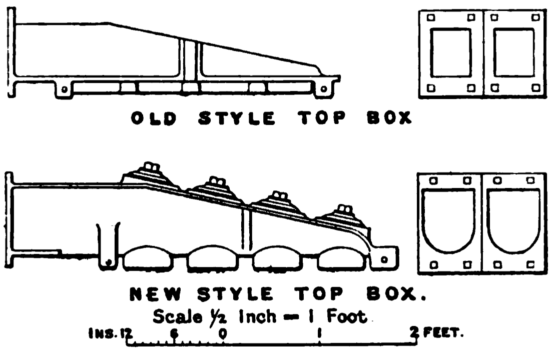
From an inspection of these curves, it will be seen that only a small proportion of the head is lost in passing through the pipes themselves, but that the boxes cause a comparatively great loss. Since these experiments have been concluded, the boxes have been considerably improved both by eliminating unnecessary sharp corners, and by greatly increasing their sectional openings originally obstructed by too wide flanges, whose adjacent sides offered a resistance to the passage of the air from the branch pipe to the boxes (see *Figs. 14*). These new boxes have also been designed of much thinner metal, with the beading of each top box running along the top side, thus changing the body from radiating to heating surface and further reducing radiation-losses.

Conclusion.—The conclusion to be derived from these experiments is, that the most efficient way of increasing the heating-value of a pipe of large bore, is to fit inside it a "winged spiral," or some similar device, which increases the heating-efficiency without unduly increasing the pressure on the fan; and further, the wings can be arranged to suit all conditions of working.

The other arrangements tried are practically unsuitable either because of want of heating-power, as in plain pipes, or because of the great pressures needed to force the air through them.

There is no material advantage to be gained by substituting thin solid-drawn steel tubes for the comparatively thick cast-iron tubes, as the rate of transmission of the heat from the flue-gases to the air is not increased. In fact, at high speeds the transmission is somewhat less with the steel tubes, because they are smoother than the cast-iron tubes, and there is less eddying. Finally in the Author's

Figs. 14.



opinion, heaters should wherever possible be arranged in series, as in *Fig. 5*.

The Author was employed by Messrs. E. Green and Son, of Wakefield, to carry out the tests described in the Paper, and he wishes to thank them for permitting him to communicate the results to The Institution, and also to express his obligations to Professor John Goodman for many and useful suggestions thereon.

The Paper is accompanied by fourteen sheets of drawings from which the Figures in the text have been prepared, and by the following Appendixes.

[APPENDIXES.

APPENDIXES.

Appendix I.—Tests on a Thirty-two-Pipe Heater fitted with Winged Spiral in each Pipe.

Air passing down sixteen and up sixteen pipes. Wings at an angle of 135° with direction of flow of air.

Number of cubic feet of air per minute entering the fan at 62° F.	360	680	926	1,150	1,380
Pressure of air in inches water-gauge at inlet to the heater . .	0·79	2·9	4·89	7·10	10·75
Pressure of air in inches water-gauge at outlet from the orifice .	0·33	1·2	2·10	3·04	4·5
Loss of pressure of air in inches water-gauge between inlet to the heater and the orifice	0·46	1·7	2·79	4·06	6·25
Temperature of air entering the heater — Dry bulb . °F.	64	62	62	54	67
Temperature of air entering the heater — Wet " . "	55	55	54	49	56
Temperature of air leaving the branch pipe . . . . . °F.	519	491	404	334	338
Rise of temperature of air in passing through the heater . °F.	455	429	342	280	271
Temperature of flue-gases entering the heater . . . . . . °F.	727	764	704	666	726
Temperature of flue-gases leaving the heater . . . . . . °F.	599	610	550	515	552
Difference in temperature between the mean temperature of the gases and the mean temperature of the air . . . . . . °F.	371	410	394	396	436
Speed of the air passing up pipes, i.e. $\frac{\text{Volume entering heater} + \text{Volume leaving heater}}{2 \times 16 \times \text{sectional area of one pipe in square feet}}$ Feet per minute	414	767	983	1,160	1,400
Number of British thermal units transferred to the air per square foot of heating-surface per hour, per 1° F. difference between the mean temperature of the gases and the mean temperature of the air	1·55	2·48	2·81	2·85	3·0
Speed of flue-gases passing the heater	250 feet per minute				

APPENDIX II.

Calculations Relating to Gas-Temperatures.

If an economizer and air-heater are to be installed together, it is necessary to calculate the temperature of the flue-gases after leaving the economizer. The following is an approximate method of calculating this temperature, the probable temperature of the feed-water leaving the economizer, as well as the size of air-heater, being required.

In the first place, under the best working conditions an economizer will impart to the water heat amounting to about 3 B.Th.U. per square foot of economizer heating-surface per hour, per 1° F. difference between the mean temperature of the gases and the mean temperature of the water.

Let N = number of tubes in economizer, then there are about $10\,N$ square feet of heating-surface.
T = temperature of gases entering economizer.
T_1 = temperature of gases leaving economizer.
t = temperature of water entering economizer.
t_1 = temperature of water leaving economizer.
W = weight of water evaporated per hour.
w = weight of coal burnt per hour.
G = weight of flue-gases per pound of coal burnt.

The difference in temperature between the mean temperature of the gases and of the water $= \dfrac{T + T_1 - (t + t_1)}{2}$.

B.Th.U. given to feed-water per hour $= W\,(t_1 - t)$.

B.Th.U. given to feed-water per hour, per 1° F. difference $= \dfrac{2\,W\,(t_1 - t)}{T + T_1 - (t + t_1)}$.

B.Th.U. given to feed-water per hour, per 1° F. difference, per square foot of economizer heating-surface $\dfrac{2\,W\,(t_1 - t)}{10\,N\,\{T + T_1 - (t + t_1)\}} = 3.$

$$\text{Therefore} \qquad \frac{W\,(t_1 - t)}{(T + T_1) - (t + t_1)} = 15\,N \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Secondly, the heat gained by the water in an economizer may be taken as being 90 per cent. of the heat lost by the gases.

The heat gained by the water per hour $= W\,(t_1 - t)$.

The heat lost by the gases per hour (specific heat of flue-gases $= 0\cdot24$)

$$= G\,w \times 0\cdot24\,(T - T_1).$$

$$\text{Therefore } W\,(t_1 - t) = 0\cdot9\,G\,w \times 0\cdot24\,(T - T_1) = 0\cdot22\,G\,w\,(T - T_1) \quad . \quad (2)$$

Now N, T, and t are known,
w can be taken as $\dfrac{W}{8}$,
and G can be taken as 20 lbs.

Equation (2) can therefore be simplified for average working conditions to :—

$$(t_1 - t) = 0\cdot55\,(T - T.) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The other data required for determining the size and style of an air-heater are as follows :—

(1) Quantity and temperature of air required.
(2) Distance of drying-room from heater.
(3) Analysis of flue-gases.
(4) Weight and specific heat of flue-gases.

Or, failing these last two

(5) Weight of fuel burnt per hour and conditions of stoking.

The number of units of heat which will have to be transferred from the flue-gases to the air are found from (1) and (2); and from (3), (4), or (5) the probable temperature of the flue-gases after passing the air-heater can be determined and consequently the difference between the mean temperature of the flue-gases and the mean temperature of the air.

The results of experiments on different types of heaters, given in this Paper, give the B.Th.U. transferred to the air per square foot of heating-surface per hour, per 1° F. difference between the mean temperature of the gases and the mean temperature of the air.

It is not, therefore, a difficult matter to ascertain the size and type of heater to do a given amount of work under any given conditions.

Regulation of Temperature.

It is easy to keep the air-temperature uniform by means of the double-duct system of air-supply, that is, there is an auxiliary supply of cold air passing along the duct, and the heated air along another, the two supplies finally meeting in a common air-duct. The supplies of hot and cold air are regulated by means of a damper in each, so connected by a system of levers that when one opens the other closes by a corresponding amount. The dampers can be worked by hand or by an automatic thermostat-arrangement; but as the flue-gas temperature does not fluctuate much, it is generally unnecessary to fit the latter.

Need of Continuous Scraping.

The Author would impress upon all users of economizers and air-heaters the necessity of keeping the pipes scraped clean of soot.

The following hypotheses have been advanced with reference to this point, namely :—

(1) That soot will not cling to pipes through which hot air is passing in the same way, and to the same extent, that it clings to pipes through which water is passing.

(2) That when the scraper-gear is at work, so much cold air leaks into the economizer chamber through the chain-holes, that it is better to work the scrapers for a short time only each day, and during the period the scrapers are not working keep the chain-holes plugged up with ganister or some other material, to prevent cold air leaking in.

With regard to the first hypothesis, a series of temperature readings was taken of the air issuing from a heater at work in Scotland, both with the scrapers working continuously, and after the scrapers had been stopped for several days. During the regular working of the scrapers the average temperature of the air entering the heater was 37·4° F., and the average exit temperature was 206° F., the rise

in temperature being 169° F.; but after the scraper-gear had been stopped for a week observations were again taken, and it was found that the rise in temperature of the same volume of air per minute was only 117° F., the efficiency of the heater having thus fallen 30 per cent. After the scraper-gear had been stopped 10 days the pipes were inspected, and it was found that the coating of soot upon the pipes was nearly 1½ inch thick, this amount being equally distributed over the whole of the heater.

With regard to the second, the Author carried out two tests upon an economizer of 256 pipes, which was at work in an electric installation in Lancashire. The first test was made with the scraper-gear working as usual, and the second was commenced immediately after the scrapers had been stopped, and the chain-holes had been plugged up, the test being continued under these conditions for a period of over 6 hours.

The following are the tabulated results of these two tests—

Conditions.	Scraper-Gear working as usual.	Scraper-Gear at work for 1 hour before commencement of test, and then stopped and chain-holes plugged up. Test commenced immediately after scrapers had been stopped.
Duration of test	6·3 hours	6·25 hours.
Temperature of water entering economizer	100° F.	88·7° F.
" " leaving "	260° F.	234·2° F.
" gases entering "	727° F.	762·5° F.
" " leaving "	358° F.	400° F.
Average difference in temperature between gases and water	362° F.	420° F.
Gallons of water evaporated per hour	1,711	1,717
Rise of temperature of feed-water	160	145·5
B.Th.U. absorbed by the water per square foot of economizer heating-surface per hour, per 1° F. difference between the mean temperature of the gases and the mean temperature of the water	2·95	2·32

It can be easily shown that it is not possible for a great quantity of cold air to leak into the flues through the chain-holes, and even in an extreme case the temperature of the flue-gases would only be lowered about 10° F.

APPENDIX III.

Test of an Air-Heater containing Ninety-six Pipes installed in an Edinburgh Laundry.

The air circulated down half the pipes, up the other half, and then passed out into use.

Number of cubic feet per minute entering fan at 48° F. .	4,074
Pressure in "inches water-gauge" at inlet to heater . .	1·38
" " " outlet from heater .	0·52
Temperature of air entering fan	48° F.
" " leaving heater	242° F.
" flue-gases entering heater	647° F.
" " leaving "	421° F.
Weight of coal consumed in 8 hours	3,495 lbs.
Heat gained by the air per square foot of heating-surface per hour, per 1° difference between the mean temperature of the gases and the mean temperature of the air	2·4 B.Th.U.
Percentage of heat lost by the gases which is gained by the air	76

(*Paper No. 3581.*)

"Bacterial Sewage-Disposal."

(*Abstract.*)

By William Ransom, Assoc. M. Inst. C.E.

The disposal of town and city sewage is one of the most important problems to be dealt with by the municipal engineer, and in recent years local authorities in different parts of the country have undertaken experiments in an endeavour to find a solution.

The question of sewage disposal has an important bearing on the provision of an adequate and unpolluted water-supply. The Rivers Pollution Prevention Acts have forced authorities to dispose of sewage without contaminating any possible source of public water-supply.

Until recently, the methods of sewage disposal in use could be roughly classified as follows: (1) discharge into a river or into the sea without treatment; (2) chemical treatment; (3) intermittent downward filtration; (4) irrigation. All these methods have failed in some important point, and the great difficulty with regard to the last three has been the disposal of the sludge. There has been much misconception with regard to the manurial value of sewage; the cost of treatment has been ignored, and expectations of vast profits from sewage farms, etc., have not been realized. It is now recognised that the manurial value of sewage is not equal to the cost of treatment.

The latest system of sewage disposal is that of bacterial treatment, and it is proposed to describe the scientific principles upon which this system is founded.

The first step in the development of the bacterial treatment of sewage was taken in 1877 by Muntz, who found that fermentation and putrefaction are due to bacterial action. Sewage applied to land had been found to undergo a process of purification, as the result of bacterial agency in the top layer of the soil. Bacterial treatment is simply an application of Nature's method of refuse-removal, whereby dead matter is rendered available for plant life by means of

bacteria. Nature is able to cope unaided with all ordinary waste and decay, but becomes overtaxed when asked to deal with the accumulated sewage and filth of towns and cities. The business of the engineer is therefore to establish such conditions as will aid natural laws to accomplish the work of sewage disposal.

The bacterial organisms thus utilized are a class of low plants, gifted with a remarkable power of multiplication if the conditions of development be favourable. Under favourable conditions, from one bacterium, 16 million organisms can be produced in one day. Reproduction and activity are, however, speedily checked if the bacteria are allowed to remain in contact with their own excretions. Bacteria can feed on a complex diet, and absorb food direct from sewage; but ordinary plants require a simple diet, and bacteria by feeding on complex matters are able to reduce these into a form suitable for plant consumption. They are Nature's scavengers, and the engineer has only to give them a suitable environment to enable them to deal with sewage disposal. The bacteria which are used may be classified under two heads, depending upon whether oxygen is necessary or not necessary for their propagation.

The bacteria which do not require oxygen are called anaerobic, and they do the work of breaking down and liquefying the organic matter, reducing solids to liquids and gases. The conditions necessary to obtain the best results are: (1) an abundant food-supply, (2) darkness, (3) equable temperature, (4) exclusion of the atmosphere, and (5) a steady current to remove products of decomposition.

The second group of bacteria has been called the aerobic, because oxygen is essential to their propagation. Aerobic bacteria build up the reduced and liquefied organic matter into nitrates. The following are the conditions necessary to obtain the best results: (1) an abundant food-supply, (2) equable temperature, (3) an abundance of oxygen, (4) removal of products, and (5) suitable material for the cultivation of the bacteria.

The effluent which is produced after these two groups of bacteria have been allowed to act is suitable for discharge into a river. It may even have a purifying effect upon river-water, because the oxygen from the nitrates is able to oxidize the organic matters present in the water. In Nature there is no hard and fast line between the action of these two classes of bacteria; they are in reality dovetailed one into the other. Experiments have been made at London, Manchester, Birmingham, and other places, to determine the best artificial conditions, and the evolution of design in connection with bacterial treatment has followed the growth of the knowledge of bacterial action from year to year. The conclusions

to which engineers are now tending with regard to bacterial treatment can be summarized as follows.

It is agreed that before crude sewage is admitted to treatment it should be subjected to a process of screening or settlement, so as to allow of the removal of such detritus as is unaffected by bacterial action. The first anaerobic stage is best accomplished in a tank called a "septic" or disintegrating tank, through which there is a continuous flow so as to carry away the products of the bacterial action. The tank should be large enough to allow the crude sewage in passing through to remain long enough in it to complete the bacterial work of liquefaction. Air and light can be excluded by means of a roof, or a natural covering of scum may be allowed to form on the surface of the crude sewage. A deposit of ash accumulates on the floor of the tank, but otherwise there is little sludge requiring removal. The effluent is then ready for the second or aerobic stage. The presence of oxygen stops the action of anaerobic bacteria but encourages the development of aerobic bacteria. Aerobic bacterial filters on beds are therefore formed of a material which will provide a breeding ground or surface, and is porous enough to contain oxygen in sufficient quantity to promote bacterial activity. The best results are obtained with a continuous automatically distributed flow which enables the beds to keep up a supply of oxygen without actually resting or allowing the bacteria to become dormant. Gravel, coke, slag, etc., form good filtering material, and will last indefinitely if well aerated by means of air-drains.

The normal temperature of sewage is suitable for promoting bacterial activity, and allows the processes to go on both in winter and summer. With a continuous flow an abundant supply of organic matter is always available, so that the development of bacteria can go on incessantly and so perform the work of sewage disposal. Conditions are then established under which the growth of the bacteria is only limited by the food-supply. That this result has been achieved is largely due to the recognition of the necessity for ensuring a continuous flow in order to remove the products of bacterial life which would otherwise suspend bacterial action. Success depends entirely upon the care taken to impose proper conditions of service upon the natural scavenging agents.

The final effluent could be discharged without danger into any river, but the Local Government Board still insist on its being passed through land before discharge into a watercourse. According to Dr. Pickard, the bacterial treatment of sewage tends to check the growth of the specific organisms of disease.

An acid sewage is difficult to treat because it checks bacterial life. It might be possible to neutralize the sewage, but a more promising method would be to introduce a "cultivation" of bacteria capable of promoting decomposition. It is possible that several vital points have yet to be unravelled, and that bacteria will play a still more important part in the disposal of sewage. It must, however, be recognized that their use has gone a long way towards solving the question of sewage disposal, and that the bacterial treatment is the best answer that has yet been given to a difficult engineering problem.

(*Student's Paper No. 597.*)[1]

"Electricity in Factories."

By Alexander Colin Anderson, Stud. Inst. C.E.

Omitting any comparison of the merits of electricity with those of other forms of power, it is proposed in this Paper to assume that the proprietor of a factory, engine-works or shipyard has decided upon the use of the former, and upon this to proceed to the technical considerations underlying the design and general lay-out of an electrical installation, without dealing with the question of cost. There is, however, one point in connection with the financial aspect which merits consideration. Frequently the cost per unit of power supplied to a motor and the cost of fuel for, say, a gas-engine, are taken as furnishing a proper basis of comparison between the two methods of driving. This is not fair to the electric machine, for although in many cases the cost per horse-power of fuel necessary for the gas-engine is lower than that of electricity supplied to the motor, the real criterion is the total running cost, and it will be found that the first cost generally, and the charges for maintenance, depreciation and attendance invariably, are less for the motor than for the gas-engine. The ease and flexibility of electric driving, with its resultant general economy, are producing a constantly increasing preference for electricity.

Source of Supply: External and Internal.—Having decided to adopt electricity, the first question that arises is whether the power required shall be produced on the premises or bought in bulk. There is a temptation to purchase the electric power, as it puts the factory manager to far less trouble, and power companies are theoretically able to provide power at the cheapest rate possible. The purchase of electricity by the factory or shipyard has the advantage of practically eliminating stand-by charges such as the

[1] This Paper was read and discussed at a meeting of Students in Birmingham, on the 21st March, 1907.

banking of fires to provide steam at short notice to cope with the peaks of a fluctuating load. Moreover, sufficient machinery must be put down in the factory generating-station to deal with the maximum demand of the works, together with spare sets to fall back upon in case of breakdown. On the other hand, a private generating plant is probably more reliable than an outside supply, which may be subjected to influences endangering the supply to the factory; so that it is usual to provide one or more spare sets to run the more important part of the works in the event of a failure of the main supply. The cheapest external power will be in general obtained from a large company, and as it will be probably supplied at high pressure, there is greater possibility of breakdown outside the works. Moreover, transformers, and in many cases rotating machinery, partly on the high-pressure circuit, will have to be employed, and this will not improve the safety-factor of the works station. Spare sets of these transforming devices are as necessary for reliable supply as are the stand-by dynamos of a private generating-plant. A low-pressure system would certainly be adopted for an internal supply, unless the supply area is very large. If the conditions under which external power is supplied are unsuited to the requirements of the factory, elaborate converting arrangements are rendered necessary, thus increasing the first cost both of the machinery and the switch-gear, and adding complications to the plant. For example, a power company's supply of alternating current may have a frequency of 25 cycles, which is suitable for power, but, as is generally conceded, too low for lighting, and therefore an apparatus must be provided for converting either the alternating current to direct current or for changing the frequency to at least 50 cycles per second. A private central station of moderate size can often generate power more cheaply than it can be purchased, for frequently in works the load factor is very good, and many plants work night and day at practically full power. Such conditions give excellent economy, and it is then best, unless exceptional cheapness of the external supply, and possibly other conditions, determine the contrary, for a works requiring 200 HP. and over, to generate its own electricity. The cost of power from an external source is rarely less than 1*d.* per unit, whereas, for example, the total works cost per unit generated in the power plants of two Sheffield works, of 640 kilowatts and 1,325 kilowatts capacity, is 0·72*d.* and 0·68*d.* respectively, and even lower figures are obtained.

Occasionally, leads are brought from an external source as a standby into a works generating its own electricity; but if the two systems be different, complications occur and special machinery is required.

Energy thus obtained will not be bought cheaply, but anything is preferable to a complete stoppage of the works.

Choice of a System.—The factory manager generally finds that he has to decide between the rival merits of a direct-current three-wire system and of three-phase alternating current. He wishes the first cost and maintenance charges to be low, but the system adopted must be as reliable and efficient as possible. It is to be feared that the adoption of electric driving in factories has been frequently retarded by the inability of the manager to decide between the claims of competing systems.

In the case of a large establishment or an amalgamation of several factories or shipyards situated some little distance apart, it is best to instal one central station, as nearly as possible at the electrical centre of the system, in which three-phase alternating current would be generated at moderately high pressure, say 3,000 to 4,000 volts, and transmitted by overhead mains to sub-stations arranged at convenient points where the pressure could be stepped down by means of static transformers. In considering whether an ordinary factory should be equipped on the three-phase system or with a direct-current three-wire installation it may be remarked that the cost of copper in the mains is, theoretically, nearly the same for a three-phase star-connected installation, and for a three-wire continuous-current system, but if the three-phase system be mesh-connected the direct current has a decided advantage in this respect. Alternating-current motors are very good for constant-speed work on constant load, as in the case of textile mills, because their speed is determined by that of the main generator. Continuous-current motors may be arranged for constant speed with varying load, but it has been found impossible, so far, to compensate for the variation of speed due to heating. Alternating motors of the type best suited for a factory do not need a commutator, a very vulnerable part of the direct-current machine; while the mechanical design of the squirrel-cage rotor of the induction motor, ensuring freedom from breakdown, gives another advantage to the alternating system. The continuous-current system gives nevertheless much better results and closer regulation with a varying load (an important consideration in view of the lighting portion of the installation) because the electromotive force of an alternating-current system is more variable, although the effect of induction motors in producing a lagging current may be considerably neutralized by the use of over-excited synchronous motors. For good efficiency with induction motors, the air-gap must be very small, and therefore there is but little margin for wear in bearings. Moreover, t e frequency for lighting should be at least 50 cycles, whereas that most suitable for

motors is 25 cycles per second or lower. The choice of speeds for alternating-current motors is somewhat restricted, and for variable speeds they compare unfavourably with direct-current machinery. Synchronous motors will not start on load and require separate excitation. Direct-current machines may be readily started and their speed is easily regulated within very wide limits, a facility enhanced by the introduction of the inter-pole design, and the use of a three-wire system which provides two pressures. All motors above 4 brake-HP., installed on the latter system, are placed across the outers, so that the switching on of a machine has no perceptible effect on the lamps. The electromotive force of an alternating-current system may be stepped up or down very easily and efficiently by means of static transformers, but this is seldom necessary in works of ordinary size. For the average factory, where a large number of variable speed motors are required, and in which generally the same system is used for lighting, three-wire direct current with 440 volts across the outers is the best; although it will be evident from the foregoing that the choice of a system is really dependent on the circumstances of the particular case.

Electrical Generating Plant and Auxiliaries.—Although dynamos are now more or less standardized, it is best to have duplicate machines, so that their several parts are interchangeable; it is also well, as an additional safeguard against undue loss of time at a breakdown, to keep in stock a spare armature, in the case of a direct-current machine, or spare armature coils for an alternating-current dynamo, together with one or two field coils and a few spare brushes. A complete standby generating set is also generally installed, for although it adds considerably to the capital cost, it is useful as the works extend, and by supplying the factory in the event of a bad mishap, will probably prove ultimately economical. A careful specification of the machines to be supplied should be drawn up, the conditions of which should be strictly complied with. Nothing should be sacrificed to cheapness, since breakdowns are very costly, and the dynamos must be liberally rated, viz., a 70° F. temperature rise after 6 hours' run at full load, which means, in the case of British machines, that an overload of 25 per cent. for 1 hour, or 50 per cent. for short periods of a few minutes, may be obtained without injury to the plant.

Direct-current machines should be fitted with commutating poles, which will ensure sparkless running even with the heaviest overload. Continuous-current three-wire dynamos may be obtained arranged on the Dobrowolski system with a balancing coil or coils, but their superiority over the ordinary type of machine with a separate

balancing set is doubtful, although, owing to additionals lip-rings and brushes, their efficiency is higher. These special three-wire dynamos can be paralleled to each other and with two-wire machines if the voltage is right, provided that suitable equalizing connections are made. Gas-driven dynamos should have special means provided to prevent unevenness of turning moment, which causes the lights to flicker and renders the machines difficult to parallel to each other. Large fly-wheels are of use in this respect. Special attention should be given to the means of synchronizing alternators coupled to gas-engines, and the mechanical synchronizers now coming into use will prove of value in this connection.

Alternating-current generators should be of the rotary-field type with stationary armatures, because the coils at the higher potential do not revolve, and being more mechanical are less liable to failure. Care should be taken to dry the machines thoroughly before starting, either by means of fires, or electrically by passing a current at low-pressure through the windings, because dynamos, etc., are liable to absorb moisture in transit. There is a difference of opinion on the question of electrically driving station-auxiliaries such as circulating- and air-pumps, mechanical stokers, etc., many considering that steam is more reliable. If, however, the motors are properly rated and cared for, electricity is quite as reliable as steam. It is, moreover, far more economical, as has been proved at the Salford Electricity Supply Works, because considerable losses by condensation occur in the long lengths of steam-piping; and, further, the electrical power taken by the auxiliaries may be measured at the terminals of the motor with the greatest ease, while it is impossible to gauge accurately that absorbed by steam auxiliaries.

In the case of an external supply, the power, on reaching the factory's premises, is transformed to the system already decided upon by means of devices such as those described in the following section.

Regulation: Transformers, Converters, Motor-Generators, Balancers, Boosters, and Batteries. (*a*) *Three-Phase System.*—The regulation of the electromotive force of alternators is important if a portion of the current is required for lighting, and various more or less satisfactory methods of compounding, to allow for the armature reaction and the drop in the line, have been devised by Mr. Heyland and others. By means of the Tirrill regulator a shunt circuit is rapidly opened and closed across the exciter field rheostat by the action of a differentially-wound relay, and the device may be set so as to provide for the inductive drop in the line. Pressure can be readily and efficiently

changed by means of oil, water, or air-cooled static transformers; and a boosting transformer, the secondary winding of which is in series with the line, may be used to compensate for line drop, although in the average factory the short distances make this unnecessary. It is doubtful whether it is better to use three-phase transformers or combinations of three single-phase transformers. The single-phase transformers are lighter, and two or, at most, three sizes may be standardized for use in groups throughout the works, but with a star-connected three-phase transformer the pressure is beneficially steadied by the action of the single magnetic circuit. The special transformers for electric welding have low-voltage high-current secondaries and present no difficulties, but all the joints on the secondary circuit should be exceptionally well made to avoid loss. Many fly-wheel storage systems have been patented recently for dealing with intermittent load on heavy duty. The fly-wheel levels up the load on the dynamos because the energy given to it by increased speed at light load is returned to the system when the peak occurs, the speed falling at the same time.

(*b*) *Direct-Current Three-Wire System.*—Unless special three-wire dynamos are employed, a balancing set or sets become necessary in order to keep the pressure constant notwithstanding the variation of load on the two sides of the system. The hand-regulated type of balancer is good, but needs constant attention to obtain close regulation; there is therefore an advantage in using cross-connected shunt or compound balancers, or possibly a combination of the two, which automatically regulate the pressure within close limits. It should be noted that balancers with cross-connected shunts are not satisfactory if used at a place where the line drop is appreciable; and if so used, they should be installed at the feeding point, for the potential on the loaded side must be raised to allow for the drop in the feeders, when the pressure on the motor side falls, thus reversing the condition aimed at in the cross-connection of the shunts.

The direct-current dynamo may be easily compounded level for the drop within itself, or may be over-compounded to compensate for fall of pressure in the lines. A compounding effect may be produced with a plain shunt machine with commutating poles by running with a lead given to the brushes. The Tirrill regulator is also of use in keeping the pressure level, and works on practically the same principle as when applied to alternators, with the advantage that the exciting voltage is the same as that to be regulated. Direct-current feeder-boosters need not be considered here as three-phase medium high-tension current would be used for large schemes

involving appreciable length of mains. It is probable, however, that a hand-regulated, or automatically reversible booster, working in conjunction with a battery, would give satisfaction. Shunt boosters regulated by hand are good, but one of the many automatic types available, whether simple or complicated, is generally preferred. Special mention should be made of (i) the combination of shunt booster and Tilney regulator, the latter being an automatic switch actuated by differentially-wound coils controlling the strength of the booster field in accordance with the load on the line, and (ii) the control of the excitation of a shunt booster by means of a carbon regulator connected with the load current, the resistance of which varies considerably with temperature, and so acts on the fields of a small exciter which supplies current for the shunt of the main booster on the Entz system.

A battery generally enhances the economy of the system, as it improves the load factor by providing work during the day and by frequently enabling a partial or total stoppage to be effected at night, thus preventing the running of plant at an inefficiently light load. Moreover it assists in keeping the pressure constant, by helping the generators on load peaks. A battery is troublesome to maintain, however, and for economical working needs far more attention than is commonly bestowed upon it. Some fly-wheel storage system will probably replace battery storage in cases where the changes of load are considerable and rapid. A shunt booster can be used connected across the line wires with a fly-wheel on its shaft; it runs as a motor at light load, increasing the speed of the fly-wheel, and as a dynamo at heavy load, the falling speed of the fly-wheel then giving out energy to the line: the necessary field regulation is obtained by means of an automatic-regulator actuated by the changes in the line current. Continuous-current motor-dynamos to reduce the pressure for electro-plating and welding are very simple and need no explanation.

(*c*) *Conversion of Three-phase to Direct Current.*—This may be readily accomplished by means of rotary converters, motor-generators or Peebles-La Cour motor-converters. In the last case the synchronous motor works half as a motor and half as a transformer, while the converter works half as a direct-current dynamo and half as a rotary converter, resulting in a lighter set than an equivalent motor-generator, although in the latter the continuous-current dynamo is quite independent, and may be compounded or otherwise regulated with ease. Conversion by means of the rotary converter is cheaper and more efficient than the other two methods, but the machine is not at its best with a frequency of 50 cycles. The

mercury-vapour converter is interesting, and not only is it very efficient, but it has no moving parts. It has not yet been widely adopted in practice, owing to the fact that a very considerable potential difference must be applied across its terminals at starting.

Distribution: Mains and Switch-gear, Meters, etc.—The generating-station wiring should be carefully carried out, and whilst all cables must be adequately protected, they should be readily accessible for inspection and overhaul. The dynamo-leads to the switchboard are generally run in concrete ducts, in trenches, or on shelves in fire-proof passages. The lighting circuit, i.e., the local circuit of the station, is best visibly supported on insulators, so that a fault is at once evident. The use of bare overhead mains is likely to be more widely adopted in this country now that the Board of Trade has intimated its willingness to consider each case on its merits, and the use of aluminium in place of copper in this connection is worth consideration, although its advantages are doubtful. With three-phase lines care should be taken to prevent the inductance of the mains affecting telephone circuits. With bare mains above ground great care must be taken to protect the apparatus from lightning. The best systems of underground lines are probably the "draw-in," or alternatively the copper strip system. The first comprises conductors with insulated sheathings drawn into ducts consisting of either bituminous or earthenware conduits or cast-iron pipes, while the second is formed of bare copper conductors laid on insulators in culverts. Cheaper methods, however, may be adopted with safety in ordinary works, as there is little danger from the labourer's pick, and the location of the mains is simply defined. The armoured-cable system, in which conductors with insulated sheathing protected by steel armouring are laid direct in the ground with a stout plank a little way above them, is also suitable. The "solid" design may be also recommended; in this case conductors sheathed with insulating material are laid in troughs of wood, earthenware, or cast iron, which are then filled in with a bitumen compound or pitch.

In an alternating system, care must be taken to diminish inductive effects as far as possible when using metallic armourings or laying cables in iron pipes. Three-core cables should be used, not triple concentric cables, and the algebraic sum of the currents must be zero, as otherwise the inductive effects of the three cores will be unequal and there will be a loss in efficiency. Moreover, in special cases, where direct-current and alternating mains may have to be laid in the same works, great care must be exercised to avoid the mutual

effects of self-induction. With concentric systems of wiring great stress should be laid on sound cable insulation, as otherwise destructive short circuits may occur. Armoured conductors should be adopted for use with portable tools; but it is somewhat difficult to combine proper flexibility with sufficiently strong and reliable armouring. Both metallic conduit and wood sheathing are in use for shop-wiring, but accessibility and visibility are of great importance, and the Author thinks that for the ordinary factory insulated wires supported on insulators are excellent. Great care must be taken when leading cables in and out of junction-boxes to avoid any possibility of surface leakage. As far as practicable, all metallic sheathings, conduits, and boxes should be efficiently connected with earth; and a coil of iron cable buried in permanently wet coke makes a very good earth connection.

The switchgear should be of simple design. The main switchboard is best arranged on the cellular independent panel principle without backs save in the case of high-pressure circuits, where the high-tension gear should be isolated in separate fireproof compartments and controlled from a distance by means of a low-tension current. The high-tension switches, which would be used only in connection with three-phase circuits, should be operated by motors and should break under oil. It is worthy of note that trouble has been experienced with direct-current oil switches, owing to the carbonization of the oil. Dynamo regulators should be made as mechanical as possible; the shunt regulators should consist of bare wire in open spirals, or wound on pots or on micanite or asbestos tubes so as to be as fireproof as practicable; and the series diverters should be of cast-iron grids or of special resistance strip. Main switches of the knife type with carbon breaks, and magnetically operated circuit-breakers, in place of fuses, should be employed for important circuits. Automatic mechanical synchronisers are supplanting the lamp and transformer method, and certainly call for less skill on the part of the attendant. Where bare overhead mains are run, lightning arresters should be provided, of either the Würtz, carbon or horn types. In the two first types the arc is extinguished by breaking across a number of special contacts and the last spreads out the flare to a length at which it cannot be maintained, an action frequently assisted by the use of a magnetic blow-out arrangement. The use of fuses should be confined to minor circuits and motor panels. Many ingenious designs are available such as the Partridge sparklet method, where escaping carbon dioxide extinguishes the arc, while in others, sand or other refractory material is employed for the same purpose. Direct-current motor starters and regulators

should be of mechanical design, the former being fitted with overload and no-volt releases and resistances of the same type as those indicated for the main dynamo regulators. Liquid starters have acquired some popularity in connection with crane motors, but if employed, they should be watched, because towards the end of a shift, after continuous use, the resistance may have dropped so much, owing to increase of temperature and evaporation, that a dangerously high current may be passed through the motor armature when switching on. Magnetically operated automatic controllers are now often used for large direct-current motors; they work through successive identical cycles of operations and are very successful. Automatic-transformers and rheostats for starting alternating-current motors are very simple and need not be described here.

Edge-wise illuminated meters are very easy to read, and the Kelvin sector voltmeter and ammeter is an excellent example of a good cheap meter. It is practically unaffected by external fields, is simple and durable, can be repaired by the ordinary switchboard attendant, and owing to its knife-edge bearing, is remarkably free from friction losses. Moreover its scale is divided into almost equal divisions. The Kelvin electrostatic meters are especially good, owing to their complete immunity from the effects of stray magnetisms.

Lighting and Lamps.—For station-lighting arc lamps would in general be used, assisted by ordinary glow-lamps, or more probably by tantalum lamps, if procurable. Mercury-vapour lamps are convenient for reading meters, especially those placed at some distance above the ground. In lighting yards, dry-docks, etc., flame arcs are suitable, and for closer work protected glow-lamps of the carbon or tantalum types connected to armoured flexible cables will be found serviceable. For lighting drawing offices, or other rooms, mercury-vapour lamps or inverted arcs with ceiling reflection give very good results; while the Nernst type gives a soft light, but has too short a life to render its wide adoption probable. The colour of the light is of considerable importance in some cases, as for instance, when used in textile mills. In shop-work arcs would be used assisted by smaller lamps, and it must be borne in mind that in certain situations enclosed arcs are essential. The slope of the roof and the nature of the ceiling have a great effect on inverted arcs, and frequent cleaning is necessary in dusty shops. Arc lamps used with an alternating current are less efficient than with a direct current, since about 50 per cent. of the light is thrown straight upwards. This objection may be overcome to some extent by employing the ceiling as a reflector. A frequency of 50 cycles per second should be adopted

for alternating-current lighting, an objectionable flicker being perceptible with 25 cycles. The Bastian mercury-vapour lamp, which can be obtained for circuits up to 250 volts, uses only about one-tenth of the energy consumed by the ordinary carbon lamp. The tantalum lamp has an efficiency of 1·7 watts per candle-power; the osmium lamp is said to consume about 56 per cent.; and the osram lamp 70 per cent. less energy than the carbon lamp.

Selection of Motors and Motor-Control.—In order to obtain a high standard of reliability, motors should be rated on the same scale as that already given for the main dynamos, viz., 70° F. rise after 6 hours' run at full load, with an overload capacity of 25 per cent. for 1 hour and 50 per cent. momentarily. Liberal rating should not, however, be carried too far. The Author is aware of cases where motors were bought on a 6-hour rating which would have been just as reliable if rated for 1 hour only, owing to the peculiar conditions of their work. "Load-factor," a loose expression, which has nothing to do with the term widely employed in central-station work, has been recently much used in relation to crane-motors. For example, a "load-factor" of 3 means that the machine is running on load for one-third the total time of working. Crane ratings are frequently too liberal: 90° F. rise for 1 hour's run, or for half an hour's run. Frequently 15 minutes would be ample, if the motor is not running during a large portion of the total time with extremely short intervals between each run. Whether a motor is to be enclosed and ventilated, merely protected or totally enclosed, should be decided by carefully considering the working conditions. A temperature rise of 90° F. after 6 hours' run on full load is permissible with a totally enclosed motor. Direct-current shunt machines are good for fairly constant speed work at variable load, although a closer regulation may be obtained by differentially winding with series turns; with a plain shunt machine having auxiliary poles this regulation may be secured by giving a lead to the brushes. Compound motors may be used with advantage where heavy starting torque is required, the series turns sometimes being cut out after starting and the machine continuing to run as a plain shunt. Series-wound motors are excellent for very heavy initial torque and automatically reach high speeds at light loads. Variable speed with direct-current motors on a three-wire system is easy of attainment, as not only are there two pressures available, but modern shunt motors with interpoles may have from 4 or 5 to 1 speed variation on the shunt alone, with good efficiency, so that there is little occasion to employ machines with two commutators to be put in series or in parallel with one another. Series-resistance control

may sometimes be used with advantage where speed reduction is required for very short periods only.

Synchronous alternating-current motors possess the advantage of practically constant speed at all loads and somewhat better efficiency than the asynchronous type. Moreover, they are slightly cheaper than induction motors, and may be used to improve the power-factor of the circuit. They will not start against load, and consequently must be run on a loose pulley in the case of small machines, or if of considerable size, they must be started by an independent motor, necessitating also a special synchronizing device: they need, too, a separate source of excitation. Induction motors have a mechanical construction, and may be started more readily than synchronous machines, but they render the power-factor of the system poor. Efficient speed variation is a very difficult problem with alternating-current machinery, and can only be obtained by pole-changing devices resulting in a few definite speeds, or by means of wasteful resistances inserted in the rotor circuit.

Miscellaneous Applications of Electricity.—The uses to which electricity may be put in factories or shipyards are many and various, and amongst these not the least important is their use for crane work. For this duty, direct-current series motors are desirable, owing to their large initial torque and their capability of automatically attaining high speed with light loads. This last advantage may be carried too far with very light loads, and sometimes a small shunt winding is put on to prevent actual racing. With large cranes, it is best to use a separate motor for each motion. Light goods-hoists are frequently worked by compound motors, i.e., really shunt machines with a few series turns to assist at starting. Motors are becoming more popular for the driving of large rolls, and 1,500-HP. motors have been installed for this purpose with success. They should be heavily compounded to help in starting, and are equipped with heavy fly-wheels to assist at times of maximum demand. Some machines have been connected directly with the rolls, but a rope-drive is frequently preferred, as it lends flexibility to the arrangement. Small machine-tools are most economically driven in groups, a motor driving the corresponding length of shafting, but for large machines, such as boring-mills, large lathes, etc., a separate motor should be used. With the high-speed tool-steels now in favour speed regulation is a matter of considerable importance, and attention is drawn to machines supplied with a motor and regulating switch as an integral part. Motors should be compounded when used with punches, shears or similar tools. Holden magnetic clutches are

neat, convenient and durable; their working depends on the attraction of two steel faces for one another, owing to the magnetic effect exercised by a coil embedded in one of them and excited from a low-tension circuit. They are readily controlled by means of a small switch. Many portable tools on this principle are of great interest. Drills, for instance, are sometimes arranged with massive iron feet containing embedded coils energized from the same circuit as the motor, and serving to hold them on the iron surface to be drilled. They are especially useful in shipyards and on structural works. The small hand-drills used in fitting shops should not be too closely rated, as they may be severely tried at the hands of an indifferent workman. Lifting magnets are very useful in steelworks or foundries, where they displace slings and chains. The magnet is suspended from the crane-hook, and is excited by a low-tension current. Special attention should be given to the insulation, etc., as a failure of the excitation generally causes a bad accident. Magnetic swarf-separators generally pay well for their installation and are extremely simple; more may be saved by proper attention to the turnings, chippings and filings of the more expensive metals than is imagined by those who neglect it. The cutting of steel piles by the electric arc is an interesting recent application of electricity. One wire of the circuit terminates in a carbon electrode and the other is connected directly with the steel to be cut; the arc is started by pushing the carbon towards the steel and care is taken that it is not broken again. The operators have to be carefully shielded by means of asbestos gloves and masks. This method has been proved in New York to effect a distinct saving both in time and in cost of cutting as compared with the ordinary practice. Pumping affords a great field for the employment of electricity, and alternating-current motors may be profitably used when the load and speed are approximately constant. If speed regulation is required, as in some classes of pumps for condensing machinery, direct-current interpole shunt motors with field rheostat regulation are suitable. For three-throw pumps driven through heavy gearing the motor should be compounded to assist when starting. Electric radiators for heating purposes are especially useful for large offices, as they need very little attention. Electric furnaces are largely used for many purposes, and are even employed in steel-making, although not as yet to any great extent. Electro-plating is now a familiar process: it is important to arrange the dynamo, switch-gear and vats as near together as possible, and all joints must be soundly made, as instances have occurred in which as much as 50 per cent. of the power developed

by the dynamo was lost between the machine and the vats owing to faulty design. Blue printing by arc light is a great convenience in drawing offices; it is both efficient and easy to regulate.

Alternating-current machines, especially induction motors, are best suited for driving textile machinery, because the load is practically constant and the speed should be constant. Due care should be taken to see that the motors are properly protected against fluff, etc. Electric locomotives are suitable for moving materials or finished parts from one section of the works to another, and the ordinary direct-current arrangement of an overhead wire with the rails as the return is serviceable enough. In driving fans, such as cupola blowers, alternating-current motors can be used if much variation of speed is not required; but should change of speed be necessary in order to regulate the quantity or pressure of the blast, direct-current shunt motors should be employed. Dynamos specially arranged as brakes have been found very useful for testing purposes in motor-car works, etc. The armature is arranged to be coupled to the engine to be tested; while the field-magnet is supported in as nearly as possible frictionless bearings, and carries an arm which can be loaded to counterbalance the drag of the armature on the field, thus giving a measurement of the torque. Eddy-current brakes, depending for their action on the resistance offered by a magnetic field to the rotation of a metal disk, are also used to some extent.

Conclusion: Maintenance, Logkeeping, Inspection, Break-downs and Repairs.—Too much stress cannot be laid on the necessity for keeping an accurate log in the works power-house in precisely the same manner as would be adopted for an ordinary central-station supply scheme; and further, a record of the condition and history of all motors and other electrical gear in use throughout the works should be carefully maintained. Thus, the actual cost of driving and lighting will be always readily apparent, weak spots will be brought to light, and opportunities of bettering the installation will be given. Economy should be not merely sustained but progressive. Keen foresight will ensure the prevention of many breakdowns, and if a reasonable stock of spare parts be kept, any ordinary repairs will prove a comparatively slight matter, with a capable staff. Great care must be taken to keep all machinery clean and in good condition, especially the commutators and brushes of direct-current machines, while the presence of oil or swarf on the windings of either alternating-current or continuous-current motors tends to destroy the insulation, and should not be allowed. Blown fuses should not be replaced by fuses of unnecessarily large size, which, although they

may render replacement less frequent, threaten the life of the motor. With small machines a hand-bellows, and with large machines a small compressor and hose, form valuable aids to cleanly working. Machines should not be run at a greater over-load than that for which they were designed, since excessive heating causes deterioration of the insulation, which consequently fails sooner or later.

In conclusion, the Author is of opinion that, all things considered, the direct-current three-wire system is the best to adopt for driving and lighting ordinary works.

OBITUARY.

WILLIAM THOMSON, *Baron* KELVIN OF LARGS, O.M., P.C., G.C.V.O., D.C.L. (*Oxon.*), LL.D. (*Cantab.*), was born at Belfast on the 26th June, 1824, and died on the 17th December, 1907. He was interred in a grave adjoining that of Newton in Westminster Abbey.

Lord Kelvin's father was a teacher of mathematics at Belfast from 1812 to 1832, when he was appointed Professor of Mathematics at the University of Glasgow, a position which he held until his death in 1849. Lord Kelvin's elder brother, James Thomson, achieved great distinction in physical investigation, and was in succession Professor of Engineering at Belfast and at Glasgow.

After studying in the University of Glasgow, Lord Kelvin entered St. Peter's College, Cambridge, in 1841, was placed second wrangler and first Smith's prizeman in 1845, and was subsequently elected a Fellow of his college. There were then no physical laboratories open to students in this country, and Lord Kelvin spent some time in Regnault's laboratory in Paris. In 1846, at the age of 22, he was appointed to the chair of Natural Philosophy at the University of Glasgow, a position which he held for 53 years. There he made laboratory work an essential part of the college curriculum. In 1896, his Jubilee as Professor was celebrated, a unique gathering of friends, former pupils and delegates of universities and scientific societies, assembling to do him honour. In 1904, he became Chancellor of the University in which he had so long taught.

During this long scientific career he carried out the remarkable series of mathematical and physical investigations which led to his recognition as the foremost physicist and most influential and accomplished scientist of his time. In the Royal Society Catalogue to the year 1883, 262 Papers are credited to him. As an engineer, he made brilliant inventions in navigation, telegraphy and electrical engineering. The originality and importance of his work was early recognized. He became a Fellow of the Royal Society in 1851, and later received from the Society the Royal and Copley Medals, and was elected President in 1890. He was President of the British Association in 1871. For services rendered in laying

the Atlantic cable he was knighted in 1866, and in 1892 he was elevated to the peerage. He was enrolled in the Order of Merit at its institution in 1902, and also received the Grand Cross of the Victorian Order. He was a Grand Officer of the Legion of Honour, a Knight of the Prussian Order "Pour le Mérite," a Foreign Associate of the Institute of France and Foreign Member of the Royal Prussian Academy. During a period in which the advance of science in all departments has been enormously rapid, Lord Kelvin held the unquestioned rank of leader.

In 1847, at the British Association, Lord Kelvin first met Joule, and by a timely interposition secured attention to a Paper by him which proved to be an epoch-making exposition of the doctrine of the conservation of energy. Joule had recently measured the mechanical equivalent of heat. Kelvin at once adopted the new views, and became one of the founders of the dynamical theory of heat. His proposal of an absolute scale of temperature removed many difficulties in the way of its acceptance. Almost simultaneously with Clausius he propounded the second law of thermodynamics, one of the great discoveries of the century. In the hands of Clausius and Kelvin, aided by Rankine, Tait and Joule, the applications of the doctrine of the conservation of energy were rapidly developed in many departments of science. Kelvin was soon led to the further principle of the dissipation of energy, announced in 1852, and to considerations as to the age of the globe. From the observed variation of temperature in the earth's strata, he concluded that the extreme uniformitarian views of some geologists were untenable and that at some period, probably not more than 100,000,000 years ago, the earth must have had a temperature too great to be habitable. Geologists are not content with this allowance and the controversy as to the question is not ended. In hydrodynamics, Kelvin developed the theory of vortex motions originated by Helmholtz. To him is due the conception of a vortex atom, which according to Fitzgerald is the most far-reaching of any hypothesis suggested as a solution of the problem of the structure of matter. Theories of the constitution of matter and the relation between ether, electricity and ponderable matter, had a fascination for Kelvin to the end of his life. His last great memoir, presented to the British Association at Leicester, was entitled "On the motions of Ether produced by collisions of Atoms or Molecules containing or not containing Electrons."

Lord Kelvin's services to navigation were very great. Led to consider the conditions for securing steadiness in the mariner's compass he realized at once that, in order to reduce oscillation, it was

a mistake to use heavy, sluggish and strongly magnetized needles. The real remedy for unsteadiness was to make the period of vibration of the compass as different as possible from that of the ship. Kelvin made the compass card as light as possible, and to render its correction easy he used short needles, feebly magnetized and of small magnetic moment. Directive force was secured by delicacy of adjustment. His form of compass, introduced in 1876, was generally adopted, and is still largely used, especially in the mercantile marine. He also invented a sounding machine in which strong pianoforte wire, with a light sinker weighing about 30 lbs., replaced the clumsy apparatus previously employed. This not only permits soundings to be taken to the greatest depths, but in depths up to 100 fathoms they can be taken while the ship is going at full ordinary speed.

In 1867, Kelvin served on a committee of the British Association for collecting and collating tidal observations. He afterwards gave much attention to problems of wave-motion and tidal theory. He invented or suggested three important tidal instruments:—a Tide Gauge for registering the form of the tidal curve at any port, from which to deduce the tidal constants; a Tide Analyser, based on an invention of his brother James Thomson, for mechanically determining the tidal constants from the tidal curve; and a Tide Predicter, which, supplied with the constants deduced by harmonic analysis for any port, traces a tidal curve for any period of time. It can be worked at such a speed that the curve of tides for a year can be drawn in the course of a morning. Kelvin suggested the tide predicter in 1872, and the machine was constructed by Roberts and Légé under his supervision. It is now at the National Physical Laboratory. A Paper[1] describing these instruments was presented by him to The Institution in 1881. In 1883 he delivered a lecture before The Institution on "Electrical Units of Measurement."

Lord Kelvin was early consulted as to the possibility of an Atlantic cable and as to the arrangements for submarine telegraphy. He had announced the law that speed of signalling would vary inversely as the square of the length of cable, and this appeared at first to render the commercial success of such a cable doubtful. The high potential currents used by Whitehouse on the short-lived cable of 1858 undoubtedly led to the breakdown of the insulation. Lord Kelvin declared that feeble currents must be used, and his invention of the sensitive mirror galvanometer and, later, of the siphon

[1] Minutes of Proceedings Inst. C.E., vol. lxv, p. 2.

recorder, overcame the difficulties of submarine telegraphy and, indeed, proved essential elements in its successful working.

He introduced the "curb" system of signalling, the effect of which is to stop the current from continuing to increase after it has attained an observable magnitude, thus permitting another signal to be sent without loss of time. Lord Kelvin took an active part in the laying of the 1858 cable, and directed the electrical arrangements on the "Great Eastern" when laying the successful cables of 1865 and 1866.

A large part of Lord Kelvin's work related to the theories of Electricity and Magnetism. The adoption of an absolute system of electrical and magnetic measurement, proposed by Gauss and Weber, was greatly promoted by his enthusiastic advocacy. About 1880 he invented the quadrant electrometer and absolute electrometer, and later, the ampere and watt balances, which have been such important aids in the advancement of electrical science. He also introduced for generating stations a recording feeder-log. A very interesting account of Lord Kelvin's work, especially of his more speculative researches, will be found in an essay [1] by Professor G. F. Fitzgerald, written for the record of the Jubilee Celebration at Glasgow.

A charter for utilizing the water-power at Niagara Falls on a large scale was obtained by Mr. Evershed in 1886, and in 1890 the problem of the method to be adopted to carry this into effect had assumed a position of great industrial importance. In order to bring to bear on the solution of the problem the best scientific and engineering knowledge then available, an open international competition was instituted, and a commission, consisting of Lord Kelvin (President), Professor E. Mascart, Colonel Th. Turretini, Dr. Coleman Sellers and Professor W. C. Unwin, was formed to consider and report on the projects submitted. A very remarkable series of hydraulic and electrical plans were placed before the commission. Very little had then been accomplished in the distribution of electrical energy except for lighting. But it soon became clear that the economical development of Niagara power could best be effected by a hydraulic station of great capacity and by electrical distribution of the energy. The hydraulic part of the problem, involving the use of turbines of greater power than any previously constructed, and ingenious and novel methods of speed regulation, was solved in the plans before the commission. The method of

[1] Lord Kelvin ... with an Essay on his scientific work, by G. F. Fitzgerald. Glasgow, 1899.

electrical distribution finally adopted, the use of high-tension polyphase alternating current of low periodicity, was only arrived at after protracted discussion. A great installation of 105,000 horsepower, delivering energy to local industries, mechanical and electrochemical, was successfully created. It has been the type on which many hydro-electric installations, in various parts of the world, have since been constructed. At Niagara alone generating stations which when complete, will supply 650,000 horse-power, are in course of construction.

A few words of appreciation must conclude this brief, and therefore, necessarily imperfect account of a great and versatile career. Lord Kelvin combined the highest mathematical ability and great originality and insight in physical research over a singularly wide field, with the power of applying his vast attainments to practical and industrial problems. He was a tireless worker and preserved to the end of his life a vivid interest in every advance in science and every new application of knowledge to practical needs. The noble simplicity and dignity of his character gained for him the affection and respect of his students, whilst all who had relations with him were impressed by the charm of his personality.

Lord Kelvin was elected a Member of The Institution of Civil Engineers on the 12th May, 1874, for eminence in the electrical and telegraphic branch of the profession. He served on the Council from 1879 to 1889, and was made an Honorary Member on the 21st May, 1889, "because of his distinguished attainments as a Physicist and of his researches on the sources of energy in nature, valuable to man for the production of mechanical effect."

JOSEPH BERNAYS died in London on the 24th December, 1906, aged 71. Of German parentage, he was born at Mainz on the 26th May, 1835, and obtained his scientific training at the Polytechnic School of Karlsruhe. Subsequently he was employed for 2 years by Messrs. Sulzer Brothers, Winterthur, and in 1858 he came to this country to join the staff of Messrs. Gwynne and Company. As their chief designer, he was engaged principally in the design of centrifugal pumping machinery, and was responsible for the ingenious perfume fountain exhibited by the firm at the London International Exhibition of 1862. In 1863 he transferred his services to Messrs. Clinton and Owens, Hydraulic Engineers, as manager, and whilst in their service he brought out the "Bernays" centrifugal pump, designed to effect economy in working and in steam consumption, the manufacture of

which was taken up by his firm. In 1868 he engaged in practice on his own account. He was appointed Consulting Engineer to Messrs. Mason and Barry, Limited, proprietors of extensive copper mines in Portugal, and superintended for them the sinking of new shafts, the erection of winding and pumping machinery, and locomotives, and the construction of reservoirs, embankments, etc., in Portugal, as well as designing extensive works at Rainham, Essex, for the manufacture of sulphuric acid on a large scale. He also acted as Consulting Engineer to Messrs. Gilbert Lawes and Company, chemical manure manufacturers, Messrs. Brunner, Mond and Company, The Bischof White Lead Syndicate, the Brimsdown Lead Company, and other large firms. In 1877 he perfected a new type of twin-cylinder marine engine, which he subsequently patented. He was consulted in connection with several important arbitrations, where his expert knowledge of the chemical branch of engineering was especially valuable.

Mr. Bernays was a member of the Society of Engineers, and of the Society of Chemical Industry, and served the office of President of the former society in 1880.

He was elected an Associate of The Institution on the 10th April, 1877, was subsequently placed in the class of Associate Members, and was transferred to the class of Members on the 1st April, 1884.

JOSEPH KINCAID was born in Dublin on the 12th November, 1834. His father, though not an engineer, was one of the early pioneers of railways, being a promoter of the Dublin and Kingstown Railway, the first line to be constructed in Ireland, and of the Midland Great Western Railway of Ireland. On account of delicate health, Joseph Kincaid's early tuition was entirely private, but he entered Trinity College, Dublin, in 1852, graduating in Arts in 1857, and subsequently proceeding to the Master's degree. On leaving college he obtained practical experience as assistant engineer on the Kingstown harbour works; in the construction of Mullaghmore harbour, co. Sligo, as Engineer-in-charge; and on other works. From 1860 to 1863, he served under Sir John Fowler and Mr. C. B. Vignoles, Past-Presidents, and was engaged at home and in Spain on the construction of the Tudela and Bilbao Railway and other undertakings. He also carried out independently the lighting of Bilbao and of Lograno with oil-gas. In 1864 he engaged in consulting practice, and was associated with many important undertakings at home and abroad. His name is more particularly

identified, however, with the introduction and development of tramways in Great Britain and other countries; he was indeed one of the pioneers of this particular branch of engineering, having even prior to the Tramways Act of 1870 constructed a horse-tramway from Dublin to Black Rock. The earlier tramways were constructed with a shallow rail spiked to longitudinal wood sleepers, but Mr. Kincaid early recognized the defects of this mode of construction and introduced an entirely metallic system, which became known by his name, consisting of "tee" or "channel" shaped rails fixed on cast-iron chairs embedded in the concrete. This system continued in use for a number of years until ultimately displaced by the "girder" type of tramway rail now generally adopted. Mr. Kincaid was amongst the first to introduce mechanical propulsion on tramways; first the steam-locomotive, then cable-traction, and ultimately electric traction. He was responsible for a considerable mileage of tramways at home and abroad, including the first cable-tramway constructed in England, the Highgate Hill line. As an authority upon tramway matters he was frequently called upon to give evidence in parliamentary committee rooms and in arbitration cases. He was retained in most of the important cases which arose under the section of the Tramways Act authorizing local authorities to purchase at the expiration of 21 years the tramways constructed by companies. He was also consulted professionally in connection with railways and gasworks in England, on the Continent and in America.

In 1892 he associated with himself as partners, Mr. J. E. Waller and Mr. Edward Manville, and subsequently Mr. Philip Dawson joined the firm, which practised under the style of Kincaid, Waller, Manville and Dawson. Mr. Kincaid died after a few days' illness at his London residence, on the 20th August, 1907, in his seventy-third year.

He was elected an Associate of The Institution on the 5th April, 1870, and was transferred to the class of Members on the 25th March, 1879.

JOSEPH LINDLEY, born in Hamburg on the 18th May, 1859, was the third and youngest son of the late Mr. William Lindley,[1] at that time Consulting Engineer to the Municipality. The subject of this notice was educated in England and served a pupilage under his

[1] Minutes of Proceedings Inst. C.E., vol. cxlii, p. 363.

father at Frankfort-on-Main. On his father's retirement, Joseph Lindley continued to act for his brothers, who were carrying on the practice, and, amongst other work, he was occupied upon the designs for the sewerage of St. Petersburg, Düsseldorf and Elberfeld. When the Elberfeld works were commenced in 1884, he became Resident Engineer for his brother, Mr. W. H. Lindley, and retained that position until December, 1888, when he was called in the same capacity to direct the construction of the sewerage and waterworks at Warsaw. He remained at Warsaw 17 years. In 1905 he was obliged to resign his post owing to failing health, and his death took place on the 20th April, 1906.

At Elberfeld he was occupied on the design and execution of the large rain-water sewers for the separate system adopted for the upper part of the city, and of the combined system of sewers carried out in the lower districts on the banks of the Wupper. At Warsaw the development of the pumping-station on the Vistula, and of the filtering and pumping-station on the upper plateau, with extensive vaulted settling-tanks and filter-beds, and the chief part of the town sewerage and house-drainage works, 105 miles in length, as well as the completion of the trigonometrical survey of the city and suburbs, were carried out under his immediate direction; and conjointly with his colleague, Mr. Alphonse Grotowski, he had the management of these works as they were completed.

The success of the works carried out under his supervision was largely due to the conscientious and painstaking attention he devoted to the tasks entrusted to him and to the energy with which he overcame difficulties of all kinds, of which the administrative formalities inseparable from public work in Russia were by no means the least. He was a good linguist, speaking and writing German and French fluently, and Russian and Polish sufficiently for the requirements of his position. He gained the esteem of the authorities for whom he was employed, and the sincere regard of all who worked with him or under him. He was a Member of the Russian Society of Waterworks Engineers and of various other Continental scientific societies. In 1894 he married Miss E. Suermondt, and leaves a widow and two children, son and daughter.

Mr. Lindley was elected a Member of The Institution on the 3rd March, 1891.

FRANCIS CHARLES MIERS, second son of the late Mr. John Miers, F.R.S., was born on the 30th June, 1821, at Concou, near Valparaiso, Chile. He was educated in England and commenced his practical training at the works of Mr. John Hague, under Mr. (afterwards Sir Frederick) Bramwell, completing it at Messrs. Miller and Ravenhill's Marine Engine Works, Glass House Fields, London. In 1842 he entered the drawing office of Messrs. Fox, Henderson and Company, Birmingham, remaining there until 1844. He was then invited by his brother, the late Mr. J. W. Miers,[1] to join him in Brazil, and in the following year they established an engineering factory at Rio de Janeiro. Between 1848 and 1853 Mr. F. C. Miers undertook the superintendence of the Ponta d'Arèa Foundry and Shipbuilding works on the opposite side of the bay; but in the latter year he rejoined his brother at their own establishment, where an extensive business was built up in the construction of machinery of various descriptions, and in the building of steamboats for river and coast service. The partners also undertook many Government contracts, including contracts for dredging, and for the erection of iron structures and lighthouses at Rio Grande do Sul and on the Abrothos reefs; the latter being erected under the personal supervision of Mr. Francis Miers. Mr. Miers returned to England in 1862, and in 1866 he founded, in co-operation with his brother-in-law, the mercantile and engineering firm of Fry, Miers and Company, London, supplying steam-vessels, machinery, and other engineering materials, for railways and other undertakings in Brazil.

Mr. Miers married, in 1850, Susan Mary Fry, who survives him. In 1886 he retired into private life. Mr. Miers died, after an illness of some months, on the 20th February, 1908, in his 87th year, at his residence, Eden Cottage, Beckenham, Kent.

He was elected a Member of The Institution on the 3rd March, 1863.

GEORGE EDWARD PAGE, who died on the 12th November, 1907, was the only son of the late Mr. George Thomas Page, of Plymouth.[2] After serving his pupilage under the late Mr. James Wilson, he was appointed to the metropolitan district of the London and South Western Railway, resigning in October, 1876, to take up an appointment on the staff of the Cape Government Railways. In

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[1] Minutes of Proceedings Inst. C.E., vol. cix, p. 429.

[2] *Ibid*, vol. x, p. 91.

Cape Colony, he was engaged principally on the Eastern system under Mr. A. E. Schmid and Mr. F. G. Slessor. He was promoted Acting District Engineer in 1878 and District Engineer in the following year, and later he acted for a time as Chief Resident Engineer of the Eastern system. In April, 1882, the construction of the East London and Queenstown line upon which he was engaged being practically completed, he left South Africa. Early in 1884 he went to India as Executive Engineer on the Southern Mahratta Railway, and held the appointment about 3 years. Having subsequently carried out a railway survey for the Pudukota State, Southern India, he accepted the position of Contractor's Engineer and Agent on an important section of the Tansa Water Works, Bombay. Mr. Page was among the few engineers chosen for a difficult survey in the Malay Peninsula for the late Sultan of Johore, and on the completion of this work, he retired from the profession and came home. For several years he was a regular contributor to the Indian technical press.

Mr. Page was elected an Associate of The Institution on the 2nd February, 1875, was subsequently placed in the class of Associate Members, and was transferred to the class of Members on the 12th May, 1891.

JAMES NELSON SHOOLBRED died on the 8th September, 1907, at Kirkwall in Orkney. Born at Bangor in 1835, his early practical experience was gained under the late Mr. Charles Blacker Vignoles, Past-President, on the construction of the Tudela and Bilbao railway in Spain. After a short period in Ireland, he went in 1865 to Liverpool, where he engaged in consulting practice. His work at Liverpool lay chiefly in the design and construction of large warehouses and industrial works, involving heavy ironwork, but he also carried out tidal survey work in the estuary of the Mersey. The experience thus gained he embodied in a Paper,[1] "On the Changes in the Tidal Portion of the River Mersey, and in its Estuary," which was read before The Institution in 1876, in which year he removed from Liverpool to Westminster.

Mr. Shoolbred's association with municipal electrical enterprise in this country began in 1888, when he carried out the electric lighting of Bradford, before which date only ten towns in Great Britain were lighted by electricity. Subsequently he acted as consulting

[1] Minutes of Proceedings Inst. C.E., vol. xlvi, p. 20.

engineer for similar installations at Brighton, Birkenhead, Accrington, Stockport, Doncaster and many other places.

Mr. Shoolbred took a deep interest in the work of the various societies with which he was connected, and contributed several valuable Papers to their proceedings. He was a Member of the Society of Arts and of the Institution of Electrical Engineers, and served on a committee appointed by the latter body to consider the question of fire risks from electric lighting. He also acted on several occasions as secretary to the Mechanical Section of the British Association.

Mr. Shoolbred was elected an Associate of The Institution on the 2nd February, 1864, and was transferred to the class of Members on the 9th May, 1876.

BENJAMIN SYKES, born in 1839, was articled in 1853 to Messrs. Park, Son and Garlick, consulting engineers, of Preston, and after completing his articles he became principal assistant to the firm. In 1865 he was taken into partnership, the firm becoming Garlick, Park and Sykes, which was changed in 1884 to Garlick and Sykes.

As a consulting engineer in active practice for a period of more than 40 years, Mr. Sykes was responsible, either alone or jointly with his partner, for many important undertakings. Sea-defence work at Blackpool, training-walls, embankments and reclamation of land in connection with the Ribble navigation scheme, and the design and construction of the Preston Dock are among the principal works with which Mr. Sykes was identified. He also carried out extensions of the Preston waterworks, the water-supply of Kirkby Lonsdale, Fylde, Padiham and Hopton, the Blackpool and Fleetwood tramroad, and a railway tunnel on the Preston and Longridge railway; and designed and built a number of public and commercial buildings in Preston and the neighbourhood. He acted as consulting engineer to several local authorities and as agent for various estates, and his services were much in request in connection with valuations and arbitrations. Mr. Sykes died at Elston, near Preston, on the 10th August, 1907, in his sixty-seventh year.

He was elected a Member of The Institution on the 27th May, 1879.

LEVESON FRANCIS VERNON-HARCOURT, born in London on the 25th January, 1839, was a younger son of the late Admiral F. E. Vernon-Harcourt and, as a grandson of the Hon. Edward Vernon-Harcourt, Archbishop of York, was cousin to the late Sir William Harcourt. The subject of this notice was educated at Harrow and at Balliol College, Oxford, where he graduated in 1861, with a first-class in mathematics and in natural science. His engineering training was obtained under the late Sir John Hawkshaw, Past-President, to whom he served a pupilage of three years between 1862 and 1865, afterwards remaining as his assistant. In 1866 he was appointed Resident Engineer on the new works at the East and West India Docks, retaining that position until their completion early in 1870. He was then selected, in open competition, for the post of county surveyor of Westmeath, but he relinquished this appointment in August, 1870, to go to Alderney as resident engineer of the harbour-works. Between 1872 and 1874 he was engaged in a similar capacity on the Rosslare harbour and railway, which now forms part of the Great Western route to the South of Ireland.

On his return to London, he carried out a survey of the Upper Thames Valley, on behalf of Sir John Hawkshaw, and in 1878, he decided to engage in consulting practice in Westminster, devoting himself particularly to matters relating to river-, harbour- and dock-works and water-supply. In 1882 he was appointed to the chair of Civil Engineering in University College, London, a position for which his scientific ability combined with wide practical knowledge admirably fitted him. He retained his professorship until 1905, when, on his resignation, he was elected emeritus professor. As consulting engineer, he acted for a number of public bodies, amongst others the Harbour authorities of Newport (Mon.), of Sligo and of Poole; and his services were frequently requisitioned in connection with parliamentary inquiries. In this manner he was associated with the Manchester Ship Canal Bill, the Ribble, Aire and Calder, and Ouse navigation schemes, the River Usk works, and other important projects. In 1896 he went out to India, at the request of the Calcutta Port Commissioners, to inspect and report upon the navigation of the River Hooghly. He was also invited by the Board of Trade to furnish reports upon various engineering schemes in this country, such as the Birkenhead waterworks in the Dee Valley.

His reputation stood high among his professional brethren on the continent of Europe, by whom his services in promoting a closer and

better relationship between maritime engineers of various countries were greatly valued and cordially appreciated. At the time of his death, he was the oldest member of council of the Permanent International Association of Navigation Congresses, upon which he had served for many years. He attended on behalf of The Institution the Congresses held at Brussels, Paris and Düsseldorf, and on the last occasion, at Milan, he was also delegate of the British Government. He served as a British member of the jury for the Paris Exhibition of 1900, and acted in the same capacity at the St. Louis Exhibition of 1904. In the following year, he presided over the Mechanical Science section of the British Association at Ipswich. In 1906 he was appointed, in conjunction with Mr. Anthony G. Lyster, to succeed Sir John Wolfe Barry and Sir Charles Hartley as members of the International Commission of the Suez Maritime Canal. For his services on an International jury in Vienna in connection with schemes for large canal-lifts, he was made a commander of the Imperial Franz-Josef Order of Austria-Hungary.

His extensive contributions to technical literature were well known to engineering readers and students. He was the author of eighteen Papers, principally on maritime engineering subjects, published in the Proceedings of The Institution, a list of which will be found under his name in the indexes to the Proceedings. For certain of these Papers he was awarded a Telford medal, a George Stephenson medal, Telford premiums and a Manby premium. He also contributed valuable memoirs to the Royal Society, the Society of Arts, the British Association, the Navigation Congresses and other bodies. His larger works include "Rivers and Canals" (1882, 1886), "Harbours and Docks" (1885), "Achievements in Engineering" (1891), "Civil Engineering as applied in Construction" (1902), and "Sanitary Engineering with respect to Water Supply and Sewage Disposal" (1907); as well as articles on River Engineering and Water Supply in the ninth edition of the Encyclopædia Britannica.

Mr. Vernon-Harcourt married, in 1870, the younger daughter of the late Lieut.-Colonel H. R. Brandreth, R.E., and leaves a son and two daughters. He died, after a few weeks' illness, at Swanage on the 14th September, 1907, aged 68. He laboured incessantly, but unobtrusively, for the advancement of his profession, and was a strong and consistent advocate for the thorough scientific training of engineers. Kind and disinterested, he was always ready to help and encourage individuals, and was unwearied in support of the causes which he had at heart. Imbued with a high sense of duty, he did

his work without expectation of honours or rewards. The integrity, simplicity and gentleness of his character was recognized no less by his professional colleagues than by his personal friends, and the news of his death called forth many expressions of sorrow and regard, from both private and official sources, at home and abroad. His deep interest in the work of The Institution was maintained to the end, and he has bequeathed to it a sum of money which, in accordance with the directions of his will, has been applied by the Council to provide for a series of biennial lectures on those branches of engineering with which he was principally associated.

Mr. Vernon-Harcourt was elected an Associate of The Institution on the 5th December, 1865, and was transferred to the class of Members on the 19th December, 1871.

FRANK WILLIAM ARNOLD, born on the 15th April, 1873, obtained his scientific training at Mason College, Birmingham, and the Royal College of Science, London, and his practical experience in the works of Messrs. William Brown, Ltd., Engineers, Birmingham. He showed considerable ability in the prosecution of his studies, and amongst his scholastic successes he gained a Whitworth Exhibition, a National scholarship and the Associateship of the Royal College of Science, besides taking honours in various scientific subjects. In 1893 he entered on a teaching career and was successively science master or lecturer at the Birmingham Municipal Technical School, Northampton Institute, Clerkenwell, Chesterfield Grammar School and Municipal Technical School, and Middlesbrough High School. In 1905 he was appointed by the Indian Government principal of the School of Engineering, Dacca, Bengal, where he found fuller scope for his abilities. He was engaged in superintending an electric installation for the college buildings, when he was taken ill, and his malady being of a serious nature, he was ordered home, but a relapse occurred at Calcutta, and he died there on the 4th July, 1907, aged 34.

Mr. Arnold was elected an Associate Member of The Institution on the 11th April, 1899. In the following year he contributed a Paper on "The Adiabatic Expansion of Wet Steam"[1] to the Proceedings.

[1] Minutes of Proceedings Inst. C.E., vol. cxl, p. 221.

GEORGE ROBERT DAVENPORT, born on the 30th March, 1861, was educated at Highgate School. He was a nephew of the late Mr. T. A. Walker, Contractor, to whom he was articled in 1878, and afterwards joined his staff. As an assistant engineer and subsequently as Agent, he continued in the service of Mr. Walker and of his successors, Messrs. C. H. Walker and Company, until his death, which occurred on the 14th May, 1907, at Buenos Ayres, from typhoid fever contracted whilst acting for his firm on the port extension works at Bahia Blanca. The works on which he was employed include the Lydd railway and Kearsney loop-line, the Elham Valley railway and the Severn tunnel works in this country, and extensive harbour, dock and other works at Buenos Ayres and elsewhere in the Argentine Republic.

Mr. Davenport was elected an Associate Member of The Institution on the 6th March, 1900.

BASIL DEVENISH-MEARES, born on the 29th June, 1868, was educated at Trinity College, Dublin, where he graduated in arts and engineering in 1891. He served under Mr. James Barton on the West Donegal Railway, and shortly after its completion in 1892 he obtained an appointment as assistant engineer on the staff of the Calcutta Port Commission. After 9 years' service under the Port Commissioners, during which period he acted for a time as Resident Engineer at the Kidderpore Docks, he was obliged to return home on sick leave. In 1902 considerations of health led to his going to South Africa, where he obtained employment on the Cape Government Railways as first Assistant Engineer on surveys and construction. He remained over 3 years at the Cape, but on the abandonment of proposed extensions he returned to India, where he was occupied as Engineer for the survey and construction of mining companies' lines in the Central States. In August, 1907, his health failed and he was obliged to go into hospital at Calcutta, where he died on the 5th October, 1907, in his fortieth year.

Mr. Devenish-Meares was elected an Associate Member of The Institution on the 1st May, 1894.

GEORGE HARRISON, eldest son of Mr. John Harrison, Treasurer of the City of Edinburgh, was born in that city on the 8th May, 1880. Educated at View Park School and at Merchiston Castle School, where he distinguished himself in mathematics, he served 3 years' apprenticeship in the engineering shops of Messrs. James Milne & Sons, Edinburgh, attending at the same time the engineering classes of the Heriot-Watt College. In 1901 he entered St. Peter's College, Cambridge, and graduated in 1904 on the Mathematical Science Tripos with second-class Honours. For some months he was engaged in research work under Professor Bertram Hopkinson at the Cambridge Engineering Laboratory, and in February, 1905, he entered the Royal Ordnance Factories, Woolwich, in the capacity of supervisor, being subsequently promoted to shop-manager in the erecting shops of the Royal Carriage Factory. Whilst superintending the testing of a steel buffer-cylinder under hydraulic pressure he sustained serious injuries as the result of the accidental bursting of a plug, and died on the 21st December, 1907, aged 27.

He was elected an Associate Member of The Institution on the 5th December, 1905.

KAN HTU, who died suddenly at Rangoon on the 25th August, 1907, aged 28, was a native of Burma, of Chinese extraction, and was probably the first Burman to be sent to England to complete his engineering education. He was educated in India at Darjeeling and at Doveton College, Calcutta, afterwards coming to England to study engineering subjects at University College, Liverpool, and at University College, London. On completing his studies, he received an appointment as Assistant Engineer in the Public Works Department at Rangoon, where he was employed in the Town Lands Reclamation division under Mr. Cecil Scott. The efficient manner in which his duties were performed brought him early promotion, and the qualities which he displayed gave promise of undoubted engineering ability. His early death was much regretted by those engineers with whom, during his brief professional career, he was brought into contact.

Kan Htu was elected an Associate Member of The Institution on the 5th February, 1907.

GEORGE JESSOP was born at Manchester in 1852. His parents shortly afterwards removing to Leicester, his father established there a general engineering business, and in 1866, the business having largely increased, the London Steam Crane and Engine Works were built in that town. After serving an apprenticeship to his father, Mr. Jessop entered the business, and on his father's death in 1883 he succeeded to the direction of affairs. In 1896 an amalgamation took place with Messrs. Appleby Brothers, London, under the title of Jessop and Appleby Brothers, Ltd., now known as Applebys, Limited.

Mr. Jessop was President of the Iron Trades Employers' Association in 1896, and when this Association was merged in the present Leicester District Engineering Trades Employers' Association he served the office of President between 1898 and 1900; he also acted as its representative on the Engineering Employers' Federation from its formation in 1898 until 1905. He died at his residence at Leicester on the 9th December, 1907, in his fifty-sixth year.

Mr. Jessop was elected an Associate of The Institution on the 4th December, 1877, and was subsequently placed in the class of Associate Members.

EVERETT WILLIAM MOORE, born at Aberdeen on the 3rd January, 1879, was educated at the Blackpool Grammar School and Rossall School, near Fleetwood. In 1896 he became a pupil of Mr. William Spinks, of Leeds, with whom he remained as an assistant until 1901, gaining experience in sewerage and sewage-disposal works in various parts of the country.

In 1901 he was appointed to the staff of Messrs. Fairbank and Son, Civil Engineers, of York, becoming in 1902 Chief Assistant to the firm, with whom he remained until December, 1906. During this time he assisted in the design of a number of schemes of sewerage and sewage disposal, amongst other places, at Marlborough, Wiltshire; Darfield and Cudworth, Yorkshire; Limpsfield, Oxted and Godstone, Surrey; and Headcorn, Kent; besides waterworks for Easingwold and District, and Robin Hood's Bay, Yorkshire; and the preparation of preliminary surveys for the water-supply of Castleford and Ossett, Yorkshire. For about 4 years he had complete charge, under the Surveyor to the Commissioners, Mr. F. Graham Fairbank, of the maintenance of an extensive system of land drainage under the Selby Dam Drainage Commissioners,

covering about 100 square miles, and carried out the necessary repairs to banks, bridges and culverts.

In December, 1906, he received an appointment in the Irrigation Department of Ceylon, and left England for Ceylon in January, 1907. At the beginning of May he unfortunately contracted enteric fever, from which he died at Colombo on the 5th May, 1907, at the early age of 28 years. He was a young engineer of great ability and promise, and at all times acquitted himself with the greatest credit of the tasks entrusted to him. In 1900 and 1901 he acted as honorary secretary to the Yorkshire Association of Students of The Institution; and it is typical of Mr. Moore that promptly on arrival in Ceylon, he enrolled himself as a member of the Planters' Rifle Brigade.

He was elected an Associate Member of The Institution on the 1st March, 1904.

ARCHIBALD CRAIG WALKER, born in July, 1877, was educated at Glasgow High School and at Glasgow University, where he graduated Bachelor of Science in Engineering. He served his pupilage to Messrs. Crouch and Hogg, of Glasgow, from 1896 to 1900, continuing with them as an Assistant until 1901, and gaining during that period sound experience of railway and bridgework and sanitary engineering.

Between 1901 and 1903 Mr. Walker was engaged on the designing and estimating staff of the Motherwell Bridge Company, and in the latter year he joined the staff of the Engineer-in-Chief of the Caledonian Railway. As an Assistant Engineer he was employed on the design of the steelwork for Glasgow Central Station Extension and the Hotel Extension. Subsequently he carried out the preliminary work in connection with the Grahamston and Grangemouth Railway.

In 1905 he was appointed Assistant Engineer in charge of the construction of the Shanghai and Nanking Railway, a post which, by reason of his exceptional mathematical ability and special experience, he was peculiarly well fitted to occupy. His early death, which occurred at Shanghai on the 16th March, 1906, at the outset of a promising career, was a source of deep regret to his friends and colleagues in the profession.

Mr. Walker was elected an Associate Member of The Institution on the 12th January 1904.

FRANK WILSHERE obtained his practical training between 1876 and 1879 as a pupil of the late Mr. C. S. Williams, at Millwall. On completing his apprenticeship, he was engaged from 1880 to 1882 by Messrs. Stanley Hall and Company, Engineers and Contractors, on railway and bridge contracts for the Great Eastern and Great Northern Railways, and on the new works of the Metropolitan Railway at Neasden.

In 1882 Mr. Wilshere accepted the post of chief draughtsman to Messrs. Dennett and Ingle, Engineers and Contractors, of Westminster, and was engaged for 4 years on the preparation of details and calculations for constructional and general engineering work under the late Mr. R. M. Ordish. Upon the death of Mr. Ordish in 1886 Mr. Wilshere was appointed Chief Engineer to the firm, and occupied this position up to the date of his death on the 24th January, 1908, in his fiftieth year. During his tenure of this appointment he acquired considerable experience in dealing with and reinstating defective works and buildings, and also in the erection of structures for carrying heavy weights. He designed and superintended the whole of the work carried out by the firm, comprising constructional work in connection with theatres, public buildings, municipal offices, lunatic asylums, hospitals, and many other important buildings in all parts of the country.

Mr. Wilshere was elected an Associate Member of The Institution on the 7th February, 1899.

WILLIAM THOMAS SUGG, the veteran head of the well-known gas-lighting firm of William Sugg and Company, died at his residence, Morningside, Clapham Park, on the 28th February, 1907, in his seventy-fifth year. The modern gas-industry was the growth of his lifetime, and his work exercised no small influence upon its development in this country. He commenced his independent career as a gas-engineer in 1860, having been trained for the profession by his father, whose assistant he had been for a period of 9 years previously. His experience thus extended over more than half a century, for he remained in harness until the last. Early in that period he acquired a wide reputation for scientific accuracy in all that related to the testing of gas, and in the manufacture, to standard, of street-lamp governors, flat-flame and argand burners and other gas-lighting apparatus. One of his well-known series of London argand burners was selected as a standard testing burner by the first London Gas Referees, whilst

the "Christiania" burner introduced by him embodied the best principles of gas-lighting of the day. With the advance of his reputation for and good scientific workmanship, his business increased apace, and on its conversion into a limited-liability company, Mr. Sugg, who continued at the head of affairs, extended its manufactures in various directions, including regenerative burners, incandescence gas-lighting, cooking-stoves, gas-fires, and every variety of fitting. He was a strong supporter of exhibitions, and his firm gained numerous awards at home and abroad. A member successively of the British Association of Gas Managers, the Gas Institute and the Institution of Gas Engineers, he was a prolific contributor to technical proceedings and journals. In 1876 he read a Paper before The Institution "On Estimating the Illuminating Power of Coal-Gas,"[1] for which he was awarded a Telford premium. Versatile, enthusiastic and hard-working, he was always ready to impart his knowledge and experience to those who sought his aid. In private life a loyal friend, he was well known and generally esteemed in engineering circles, where his death caused deep and wide-felt regret.

Mr. Sugg was elected an Associate of The Institution on the 6th May, 1862.

*** The following deaths have also been made known since the 7th December, 1907:—

Members.

APPLEBY, CHARLES JAMES; *died* 26 April, 1908.
AYRES, ARTHUR; *died* 19 December, 1907.
BASSET, WALTER BASSETT; *died* 27 May, 1907.
BOYLE, RICHARD VICARS, C.S.I.; *died* 3 January, 1908.
CARGILL, THOMAS; *died* 24 December, 1907.
DANBY, WILLIAM; *died* 12 February, 1908.
GOTT, CHARLES; *died* 4 March, 1908.
HAY, GEORGE; *died* 6 February, 1908.
HETHERINGTON, JOHN MUIR; *died* 25 February, 1908.
JAMES, JOHN WILLIAM; *died* 10 January, 1908.
LITSTER, DAVID MICHAEL.
LUNDIE, CORNELIUS; *died* 12 February, 1908.
MARSH, THOMAS EDWARD MILLES; *died* 19 December, 1907.
PRESTON, ALFRED ELEY; *died* 13 February, 1908.
REID, GEORGE LOWE; *died* 7 December, 1907.
SELLERS, COLEMAN; *died* 28 December, 1907.
SIMPSON, JAMES THOMAS; *died* 27 December, 1907.
SWAN, *Col.* HENRY FREDERICK, C.B.; *died* 25 March, 1908.
SWINDELLS, RUPERT; *died* 27 February, 1908.
THWAITES, WILLIAM; *died* 19 November, 1907.
WOODCOCK, WILLIAM HUGH; *died* 28 March, 1908.

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[1] Minutes of Proceedings Inst. C.E., vol. xliv, p. 151.

Associate Members.

ABERCROMBIE, FRANCIS; *died* 20 March, 1908.

ADAMES, CORNELIUS GEORGE; *died* 28 February, 1908.

ALDIS, BASIL STEADMAN; *died* 14 April, 1908.

ARNEIL, JAMES ABRAM; *died* 1908.

BRASSINGTON, JOHN WATTS; *died* 1 April, 1908.

DARWIN, SAMUEL BROTHERS; *died* 12 February, 1908.

DAVIES, MORGAN WILLIAMS; *died* 14 February, 1908.

MACBONE, GRIEVE; *died* 27 January, 1908.

MILLER, ROBERT FAULDS; *died* January, 1908.

MYBURGH, HENRY HAWKINS.

NETTLETON, HUGH; *died* October, 1907.

OGILVIE, ROBERT CHARLES FREDERICK; *died* 4 April, 1908.

PARKES, THOMAS FARMER; *died* April, 1907.

VEEVERS, HARRISON; *died* 12 November, 1907.

Associates.

BAIRD, *Col.* ANDREW WILSON, R.E., C.S.I., F.R.S.; *died* 2 April, 1908.

FERRIÈRES, *Baron* DU BOIS DE; *died* 16 March, 1908.

SQUIRE, JOHN BARRET; *died* 22 December, 1907.

TYLER, *Capt. Sir* HENRY WHATELY, R.E.; *died* 30 January, 1908.

Information as to the career and characteristics of the above is solicited in aid of the preparation of Obituary Notices.—SEC. INST. C.E., 30 April, 1908.

Sect. III.

ABSTRACTS OF PAPERS IN SCIENTIFIC TRANSACTIONS AND PERIODICALS.

Engineering Works and Surveys in the Philippine Islands.

G. R. Putnam.

(Engineering News, New York, 20 June, 1907, pp. 691–4.)

This long article begins with outlines of the history, physical features, climate, native and foreign populations with their fitness for labour, and administration under the United States Government. The works more particularly alluded to, and generally described, are as follow:—(1) The harbour of Manila, begun by the Spaniards, the completion of which has cost £791,666 13*s.* 4*d.*; (2) the addition of 723 miles of railway, for which concessions have been granted, to the 120 miles northward from Manila, which had been constructed by an English company before the American occupation. The geographical features are such, there being 11,500 miles of coast-line to an area of 115,000 square miles, that water-carriage must always be the chief means of transport; (3) the Benquet road, a mountain work of considerable magnitude, comparable with Swiss roads of a similar character; (4) 40 miles of electric tramway in Manila, and a power-house equipped with turbine-engines; (5) water- and sewerage-works by the Municipality of Manila, now under construction, to cost £833,333 13*s.* 4*d.*; (6) the military and naval works, there being included in the latter the great floating dry-dock "Dewey," which was successfully towed from Chesapeake Bay to its destination at Olongapo; and finally (7), the Marine Survey now in progress, which, in addition to that carried out by the British and the Spaniards, makes about one-quarter of the coast-line charted sufficiently for present needs.

C. O. B.

Maintenance of Road- and Street-Surfaces.

Lieut.-Col. G. Espitallier.

(Bulletin de la Société d'Encouragement pour l'Industrie Nationale, Paris, May, 1907, pp. 542–59.)

The magnificent national roads of France which the invention of railways converted into archaic monuments, kept up from a feeling of piety when they no longer served any useful purpose, have suddenly acquired a new interest and value with the advent of the horseless vehicle, but the renewed vitality of the abandoned roads seems to be incapable of resisting the excess of activity of the motor-car, which would appear to have been introduced in order to destroy the roads made on the old system. The automobile demands a smooth and regular surface, which requires great care in maintenance, but at the same time this mode of traction imposes excessive difficulties in road-repairs owing to its destructive effect on macadam. The Author considers the various systems of road-making and the employment of granite sets, asphalt and wood blocks, and he explains the advantages and defects of each material. A review is given of the employment on roads of tar and oily matters, and of the use of petroleum, costing 16*s.* a ton in California, on the roads of Los Angeles. The best mode of applying tar is described by reference to illustrations of tar-spreading plant in actual use. It is claimed that tar must be applied very hot, on well-cleansed and thoroughly dry roads, in quantities of not less than ½ lb. per square foot. Well-tarred roads, if the macadam be thoroughly sound at the time the tar is used, will not require another coat for 12 months, or three coats in 4 years, and there is a material saving in the expenses of upkeep due to the avoidance of the cost of watering and slopping. The question of the actual cost of the application of tar, and its influence on necessary repairs and on the life of the road, is considered in respect of figures obtained in recent practice. It is stated that tar should not be employed on roads where the gradient exceeds 3 per cent., as the foothold is so insecure. An account is given of the use of so-called "armoured asphalt" in Paris, and of the excellence of the street-surface thus obtained by the use of small granite pyramids embedded in asphalt, having a total thickness of 1·57 inch to 2·75 inches varying in accordance with the density of the traffic on the thoroughfare in question.

G. R. R.

New Bascule Bridges.

(Engineering News, New York, 18 July, 1907, pp. 56–7.)

There are three types described, the Rall, the Scherzer, and the Page. An example of the first is where the inter-urban lines of the Illinois traction system enter the city of Peoria, Illinois, by a long

steel bridge over the Illinois River, with a bascule deck-span of 141 feet, giving a headway of 10 feet 6 inches above high-water. This is the second of this type to be finished. In this system the pivot or point of support moves backward from the opening as the leaf rises, and a minute illustrated description is given. Four other railway-bridges on this system are under construction. The advantages are: (1) the length of span is a minimum and equal to the distance between centres of channel-piers; (2) the length of travel is a minimum, the tail-end just clearing the channel-pier when the bridge is fully open; (3) the shop-work and the operating mechanism are simple, and the carrying-wheels can easily be removed and repaired as the bridge does not rest on them when closed; and (4) this type is very economical both as to quantities and cost.

The example of the Scherzer bascule in the same town is a double leaf of 142 feet centre to centre, which, with the operating machinery, is described. More space is given to the Page-system bridge for joint use of three railways over the South Fork, Chicago River, Chicago, it being the first of this type used for a railway-bridge. There being eighty trains a day over the river, the erection-work, which, with the principle of the system, is fully detailed, involved some novel requirements.

C. O. B.

Railway-Bridge over the River Neckar at Heidelberg.

G. LUCAS, D. R. MÜLLER and G. TRAUER.

(Deutsche Bauzeitung, Berlin, 1907, pp. 378–82, 385–7, 400–1, 407–10, 418–23.)

The Authors, in a series of articles, give a detailed description of the design and construction of a new steel railway-bridge over the Neckar at Heidelberg.

The bridge is formed with four openings, that over the main stream has a span of 231 feet, and the three others, two on the left and one on the right, have each a span of 162 feet. The river authorities demanded a clear headway of 19·5 feet above the highest navigable water-level of the river. The pier-foundations were sunk 21·25 feet into the bed of the river, and were built within steel coffer-dams, as the driving of wooden ones to this depth was found to be impossible. In order to insure the water-tightness of the coffer-dams, secondary wooden dams 13 feet from the steel dams were driven to a shallow depth. The concrete bottom was made in the proportion of 1 : 5 : 8 and the piers themselves were made of concrete 1 : 3 : 6 faced with a heavy facing of dressed Neckar sandstone. The pier-footings were protected by heavy stone-packing to a depth of 5 to 6·5 feet below the crest of the coffer-dam. The abutment-piers were constructed on similar principles, and were securely anchored to the shore embankments with bar- and angle-iron lattice-work.

The most unfavourable loads (wind- and brake-stresses) on the foundations of the piers vary between 3·85 and 1·5 tons per square

foot, and at the abutments between 2·0 and 2·2 tons per square foot.

For driving the coffer-dams hand- and steam-drivers were used, the steam-drivers having a monkey-weight of 1,550–1,750 lbs. The steel coffer-dams, consisting of standard I sections, were driven to the depth of 18·5 feet at the rate of 7 feet or 128 square feet per day of 12 hours, whereas by the wooden ones a rate of only 20 feet or 72 square feet per day to a depth of 3·6 feet was attained. The steel structure itself consists of four bowstring lattice-girders with suspended three-rail way. The total depth of the steel structure of the wide span was 39·25 feet in the middle, 20·5 feet at the end, the smaller spans being 32 and 20·5 feet respectively. The upper booms are of box-girder cross-section, whereas the lower booms have an open double II section, and the suspension members consist of four standard angle-irons 3 inches by 3 inches by 4 inches. During erection a collapse of the middle girder occurred due to the sudden sinking of the false-work and staging in the river. The damage was not very severe, for of the 182 tons of steelwork affected only 45 tons had to be replaced.

The cost of the bridge was as follows:—

	£
Foundation, pier and abutments	7,363
Steelwork, 696 tons	12,969
Preparatory work, borings, designs, etc.	1,468
Superintending building, etc.	1,793
	23,593

The article is illustrated in a very full manner by details of the bridge and photographs of the erection, as well as by numerous tables of calculations.

F. R. D.

Chippis Reinforced-Concrete Railway-Bridge over the Rhone.

(Schweizerische Bauzeitung, Zürich, vol. xlix, pp. 307–9. 13 Figs.)

This is a bridge of unusual design which has just been constructed to carry a railway of ordinary gauge over the Rhone at Chippis for the Neuhausen Aluminium Company. Owing to local conditions the bridge had to be carried in a slanting direction across the river, so that the width between buttresses was 193·5 feet. The level of the rails was determined by the level of the site of the existing works on the banks of the river, and as a clear height of 3·28 feet had to be left between the underside of the bridge and the level of flood-water in the Rhone the depth of the bridge-platform was limited to 2·3 feet; no pier could be built in the river-bed, so that a bow-string construction with suspended platform had to be selected.

The Author states that this is the largest span railway-bridge ever built in reinforced concrete, and cites earlier examples on the Continent of Europe. The conditions of loading were those produced

by a moving train with a locomotive weighing 30 tons, having a wheel-base of 11·48 feet and wagons of 30 tons with a wheel-base of 13·12 feet.

The bridge was designed by Froté, Westermann and Co., of Zürich, and has two bow girders of reinforced concrete from which the platform of the bridge is suspended. This platform is also built of reinforced concrete. The calculations for the design were based on the formula of Professor Mörsch.[1] Dimensioned working drawings are reproduced in the original, from which it appears that the height of the bow girders at the buttresses is 8·5 feet and the width 3·94 feet, while at the crown it is 4·92 feet by 2·6 feet, and the distance between centres of the two girders is 15·7 feet. The stresses and factors of safety are set forth in tables and the drawings give details of the armouring. The tests gave satisfactory results, and the bridge is now in constant use. E. R. D.

Bracing of Trenches and Tunnels, with Practical Formulas for Earth-Pressures. J. C. Meem.

(Proceedings, American Society of Civil Engineers, New York, vol. xxxiii, pp. 599–621.)

The writer states that the observed results of earth-pressure in excavation are not in accord with theory, which assumes that it is greatest at the foot of the trench, for it is observed that well-braced trenches invariably show a heavier pressure near the top than at the bottom. He develops a theory in which the pressure on a vertical side of a trench is that due to a triangular mass of earth included between the vertical side and a line drawn from the bottom of the trench, making an angle with the horizontal equal to the angle of repose of the earth, and the centre of pressure against the vertical side is where a perpendicular from the centre of gravity of the triangle intersects the vertical line. Assuming a trench 40 feet deep, he works out, on the above hypothesis, the sizes of the bracing necessary to withstand lateral earth-pressure. In the case of earth saturated with water, though the full hydrostatic pressure may not be developed, he does not neglect this in making calculations, and implies that it ought to be taken at its full value.

The pressure on the walls of a tunnel is also deduced according to assumptions similar to those for open trenches, and then various forms of bracing are discussed and illustrated by drawings. In the subway work at Brooklyn a type of bracing was employed by the contractors in which the heavy main-brace, called the middle-brace, was placed as nearly as possible at what was considered to be the position of greatest pressure, i.e., near the top of the trench, and this arrangement was adhered to, notwithstanding the fact that it spanned a continuous excavation for a six-track subway.

Two general types of tunnel-bracing are discussed. In the crown-

[1] *Schweizerische Bauzeitung*, vol. xlii, p. 83.

bar system it is customary to drive the bottom drift ahead of all other work and follow this closely with a top drift. The bars are placed in position in this top drift, the poling-boards are then driven off, and the bracing erected. The disadvantages of these types of bracing are that so much space is required beyond the limits of the structure, and that it is impracticable to fill the voids over the masonry. Another drawback is the prolific cause of settlement. Where arch-timbers are used in connection with the crown-bar system it is customary to run two bottom drifts ahead, setting in supports for the wall-plates before the upper excavation has been made. The Paper is illustrated by drawings and photographs.

R. S. B.

South Australian Railways.

(Report of the Commissioners for year ending 30 June, 1907.)

	1907		1906	
Mileage—				
5-foot 3-inch gauge		594½		507¾
3-foot 6-inch gauge		1,238		1,238

86½ miles light line (41 lbs. second-hand rails) were constructed during the year.

		£		£
Gross revenue		1,575,368		1,349,765
Working-expenses		868,005		764,385
Net revenue		707,363		585,380
Number of passenger-journeys		11,497,802		10,715,343
Tonnage of goods and live-stock		2,042,939		1,732,436
Train-mileage		4,334,243		3,875,167
Percentage of working-expenses to revenue		55·10		56·63
	£	d.	£	d.
Earnings per open- and train-mile	868	87·23	773	83·59
Expenditure per open- and train-mile	478	48·06	438	47·34
Detail of working-expenses per open- and train-mile:—	£	d.	£	d.
Maintenance	125	12·57	111	11·99
Locomotive-power	160	16·13	163	17·66
Carriage- and wagon-repairs	39	3·94	37	3·94
Traffic	95	9·51	90	9·75
General	10	0·99	10	1·08
Special (written off capital account for condemned rolling-stock and structures)	49	4·92	27	2·92
	478	48·06	438	47·34
Average mileage per ton, goods and live-stock		117·41		118·38
		d.		d.
Average receipts per ton-mile		1·08		1·07
		£		£
Capital expenditure		13,724,301		13,610,520
Net return per cent.		5·16		4·30
Rolling-stock—				
Locomotives		328		327
Coaches		429		423
Wagons		6,361		6,382

The above particulars are exclusive of the Palmerston and Pine

Creek line, an isolated railway in the north of Australia, for which the following figures are given:—

	1907	1906
	£	£
Revenue	14,018	14,897
Expenditure	13,280	13,854
Net Revenue	738	1,043
Receipts per mile open	96	102
Train-mileage	30,901	30,461

The Report states that the earnings are the highest on record. A minimum wage of 6*s*. 6*d*. per day was introduced with the year 1906–07, and the 8-hour system was extended to several additional stations. In addition to the usual diagrams and maps, photographic reproductions are given of rolling stock built in South Australia and Victoria.

C. O. B.

Victorian Railways.

(Report of the Commissioners for year ending 30 June, 1907.)

	1907	1906
Mileage on 30th June, 1907:—	Exclusive of Street Railway.	
5-foot 3-inch gauge	3,314·34	3,316·37 [1]
„ „ sidings	579·07	574·61 [1]
Equivalent to single line, including sidings	4,218·00	4,222·33 [1]
2-foot 6-inch gauge	81·60	81·60
„ „ sidings	4·75	4·71
Equivalent to single line, including sidings	86·35	86·31
	£	£
Gross revenue	4,012,641	3,787,619
Working-expenses	2,076,673	1,999,023
Net revenue	£1,935,968	£1,788,596
Passenger-journeys (about 93 per cent. suburban)	69,920,583	65,088,394
Goods-tonnage	3,650,538	3,376,987
Live-stock tonnage	315,254	299,030
Train-mileage	10,035,914	9,392,069
Revenue per open-mile	£1,182	£1,116
„ „ train-mile	95·96*d*.	96·79*d*.
Working-expenses per open-mile	£612	£589
„ „ „ train-mile	49·66*d*.	51·08*d*.
Percentage of working-expenses to gross income	51·75	52·78
Detail of working-expenses per train-mile	*d*.	*d*.
Maintenance	14·10	14·62
Traffic, including compensation	14·19	15·03
Locomotive running	12·46	12·30
Rolling-stock repairs	7·74	7·84
General	1·17	1·29
Total	49·66*d*.	51·08*d*.

[1] Electric Street-Railway included in 1906.

	1907 Exclusive of Street Railway.	1906
Capital expenditure	£41,586,076	£41,398,037
Rolling-stock, 5-foot 3-inch gauge—		
Locomotives	490	504
Coaches	1,199	1,198
„ joint stock, S.A.	18	18
Trucks	10,159	10,391
2-foot 6-inch gauge—		
Locomotives	7	7
Coaches	16	12
Trucks	106	99
Motor-buses	6	6

ELECTRIC STREET-RAILWAY.

Mileage	5·13	4·07
Sidings	0·47	0·36
	5·60	4·43
	£	£
Revenue	9,590	1,449
Working-expenses	7,451	1,141
	£2,139	£308
Percentage of working-expenses to revenue	77·70	78·74
	£	£
Cost of construction	38,635	25,013
„ rolling-stock	14,304	3,597
Excess of working-expenses and interest over receipts	9,782[1]	136

The Report deals with the great increase of traffic due to good seasons and the construction of new rolling stock, especially locomotives built locally, of which it has been decided to supply twenty per annum, the cost being £43 15*s.* to £52 4*s.* per ton.

Ten photographs of new coaching-stock accompany the Report.

C. O. B.

German Colonial Railways in Africa. Dr. R. HENING.

(Archiv für Post und Telegraphie, Berlin, 1907, p. 369.)

The Author discusses the relative advantages of the route to the west of Lake Tanganyika through the Congo State, and that to the east through German East Africa, for the Cape to Cairo railway and considers the latter preferable. He then describes what Germany has done and thinks that it compares unfavourably with the progress made by other nations. The total length of the German railways already constructed is only 946 miles, of which 385 miles have been built by private companies and are owned by them, while between

[1] £9,941, spent in making good loss by fire in Power-House were debited to Working Account.

the years 1900 and 1906 the British have constructed 4,650 miles in their Colonies. The various German railways are as follows:—South West Africa 680 miles, Togo 103 miles, East Africa 96 miles, the Cameroons 26·7 miles. Of the South West Africa line 358 miles were built and are owned by the Otavi Mines and Railway Company. Cape Colony owned in 1879, before the division of Africa, 955 miles of railway, and therefore more than all the German Colonies in 1907. The Otavi line begins at Swakopmund and runs north-east to Tsumeb. Construction work was begun in October, 1903, and was finished in August 1906; a branch line to Grootfontein in Ovamboland is to be made at once. The second line in this colony is 237 miles long, runs from Swakopmund through Karibib to Windhuk, and belongs to the Government; it is proposed to extend it 61 miles to Rehoboth; this is the most important of the lines from a commercial point of view, and may be extended to the boundary of Bechuanaland to connect with the British line. The third line is 85 miles long, runs from Lüderitzbucht to Kubub and Ans, and will soon be extended to Keetmanshoop. In Togo the coast-line between Lome and Anechs, 82 miles long, was opened in 1905, while that between Lome and Paline, 75 miles long, was opened in January, 1907. In German East Africa the Usambara line has at present only been constructed between Tanga and Mombo, a distance of 80 miles. The Author deplores the slow progress of railway-construction in the German Colonies as compared with that in the British colonies.

E. R. D.

New York, New Haven and Hartford Railway Electric-Traction Installation.

(Street Railway Journal, New York, 1907, pp. 242–54, 278–85, and 308–16.)

This exhaustive description of a most important electric-traction installation on the single-phase system, extends over three issues of the journal. In the first is the introduction, dealing with the history of the project, the reasons for the selection of single-phase traction, owing to its simplicity, economy and local suitability, and the commercial aspect. The details follow, consisting of (1) overhead construction, including intermediate bridges, anchor bridges, catenary cables and insulators; (2) rail-bonding; (3) locomotives and their electrical equipment and performances; and (4) the power-house. The illustrations are very numerous. There is a plate showing longitudinal sections of the locomotive.

C. O. B.

New Dynamometer-Car, Pennsylvania Railway.

(Railway Age, Chicago, 5 July, 1907, pp. 7–11.)

This car is fitted with apparatus to measure draw-bar stresses to 100,000 lbs. The general structure of the vehicle itself is explained, with its dynamometer-compartment at one end, and computing-compartment at the other. Briefly, the action is: a thrust or pull on the coupler is transmitted through the draw-bar to the piston of a hydraulic cylinder situated within the centre sill about the middle of the car. Pressure in this is transmitted to a smaller recording-cylinder within the car by means of oil with which both cylinders and connecting-pipes are filled. Movement in the recording-cylinder is resisted by calibrated helical springs, the deflection of which (and the resulting movement of the piston) is proportionate to the load on the coupler. A pen, actuated by the piston-rod of the recording-cylinder, marks on a strip of paper, moving beneath it, an irregular line, of which the distance from the datum line is proportioned to the load on the coupler. The weight of the car is 62 tons.

C. O. B.

Bothwell Locomotive.

(Railway Age, Chicago, 2 August, 1907, pp. 156–7.)

This ingenious and novel machine has had a successful trial. The object is to change, without stop, the arrangement of wheels suitable to easy gradients and fast running, to that required for economically mounting inclines as they are encountered. Normally, the engine is driven by four 56-inch coupled driving-wheels. Between and immediately following these are four 32-inch wheels which, when a severe incline is approached, are substituted for the others by an ingenious mechanism consisting of a shifting-gear fully explained with diagrams. This gear also lifts the ordinary driving-wheels clear of the road, and transfers the weight borne ordinarily by them to the smaller ones. The shifting-device is actuated by a 10-inch by 29-inch cylinder operated by steam or air as may be desired. Reference to the illustrations is necessary to understand the operation. On trial on a gradient, the load was increased by the change from 25 to 45 cars.

Though not included in the engine already constructed, it is contemplated to assist adhesion by special gearing, not only bringing the four leading bogie-wheels, but also the eight wheels of the tender, into driving service.

C. O. B.

Mallet Sixteen-Wheeled Compound Locomotive.

(Railway Age, Chicago, 9 August, 1907, pp. 189–91.)

This, the heaviest and most powerful locomotive yet constructed, is the first of three built for the Erie Railway. As regards flexible joints to high- and low-pressure cylinders, receiver and exhaust-pipes, articulated connection between frames, boiler, bearings, power, reversing-gear, etc., it is comparable with the smallest engine supplied to the Baltimore and Ohio Railway 3 years ago, none of these features having failed to give satisfaction. The dimensions, weight and power, of the Erie engine are, however, much greater, and they have two pairs of additional driving-wheels. Details of these dimensions, etc., are given, among which it may be noted that the total weight on the 0.8.8.0 wheel-arrangement is 410,000 lbs., the average load per axle being kept down to 51,200 lbs., which is less than that of many smaller locomotives now in use. The tractive effort, working simple, is 94,800 lbs. The four pairs of forward driving-wheels are equalized together on each side, and cross equalized in front of the forward driving-wheels, making this system equivalent to a single supporting point. The rear engine, on the other hand, is equalized throughout on each side only, without cross equalization. This forms a complete three-point suspended engine, or the best obtainable condition for flexibility and ease on the line. There are many other features of interest.

C. O. B.

Swiss Government-Railway Locomotives with Superheated Steam.

M. Weiss.

(Schweizerische Bauzeitung, Zürich, vol. l, p. 55. 1 Plate and 4 Figs.)

In September, 1906, the Swiss Government Railways ordered at Winterthur one experimental locomotive fitted with a superheater, and this gave such satisfactory results that an order was placed with the same works for twenty more of a similar type. The superheater is of the form designed by Wilhelm Schmidt, of Wilhelmshöhe, Cassel; it had been used in 968 locomotives in various countries up to April, 1907, and at that date 1,787 more locomotives were being so fitted. Of this total 1,320 were in use or on order for Germany. The Swiss locomotive is for passenger-traffic and of the six-coupled type with a two-wheeled bogie in front and a six-wheeled tender. The illustrations show a general view of the engine and also dimensioned sections with separate views of the superheater.

The chief dimensions are: cylinders (two) 21·26 inches in diameter by 23·6 inches stroke, driving-wheels 59·84 inches in diameter, wheel-base 6 feet 8·7 inches. The steam-pressure is 180 lbs. per square

inch. These dimensions are very similar to those of the three-cylinder compound engines, of which the Government railways already own 147.

The Author, however, considers the new type of engine simpler to manage than are compound locomotives. The superheater consists of eighteen fire-tubes of 4·92 inches diameter fixed above the ordinary tubes, and in each of these tubes is fixed a superheater consisting of a tube 1·26 inch in diameter inside, and 1·53 inch in diameter outside, bent on itself so that there are four of the small tubes in each of the large ones. The steam is superheated to 323° or 350° C., but this can be regulated by gearing under the control of the driver. Diagrams are given of an experimental trip with an express train, and these show that the temperature of the saturated steam was 188° C., while the temperature in the cylinder steam-chest was 330° C., the speed reached 48·4 miles per hour on the level and about 43·4 miles on a gradient of 1 in 143. The Author considers that these locomotives are likely to prove more satisfactory than the compound types in use previously.

E. R. D.

Tests of Live Load on Locomotive Driving-Springs.

(Railway Age, Chicago, 2 August, 1907, pp. 150–2.)

Frequent replacing of broken springs has led to the design of this apparatus. It consists of (1) a recording-device, which fits on a spring band and contains a roll of metallic-faced paper on which the record is made; (2) a beam or spanner-bar which is fastened to each end of the spring link-hangers and is connected to the recording-apparatus; and (3) a battery-box with storage-battery, dry batteries, rheostat, switches, keys, clock, galvanometer, and all the necessary controlling-devices. These are contained in a box fixed above the spring. In the detailed description of these which follows, it is noticeable that the spring controlling the main stylus always works in tension, which, owing to excessive vibration and jarring, is found to work better than if it were in compression. Besides the main stylus which records the deflections, there are the zero stylus, giving a datum line, another recording by code the points and crossings, curves, bridges, application of brakes, etc., while the fourth marks off 15-second intervals of time.

The application of the tests to specified runs is then explained and results are given, points and crossings producing the greatest effects. Illustrations of the apparatus, a diagram with deflection curves and six records are given.

C. O. B.

Experiments with Locomotive-Chimneys and Blast-Pipes.

E. Höhn.

(Schweizerische Bauzeitung, Zürich, vol. l, p. 10. 5 Figs.)

The experiments described in the original article were carried out by the Swiss Government Railways department at the Reil workshops. The first series were made in 1905, and showed that the best results were obtained by the use of a chimney having a cylindrical extension into the smoke-box and with its external portion slightly tapering; the blast-pipe to be as broad as possible and carried up to the centre line of the boiler. The high coal-consumption of a tender locomotive in 1906 gave rise to further experiments, and two sectional views of the smoke-box are given, one showing the original arrangement and the other the arrangement after alteration. The alteration comprised the extension of the chimney downwards and the use of nine different forms of blast-pipe, all of which are shown in dimensioned cuts. A U-shaped water-column was used for determining the pressure in the smoke-box and a mercury gauge for determining the back-pressure at the exhaust of the cylinders. The results are set forth graphically in two diagrams and numerically in a table. The Author describes the experiments in detail and concludes that the blast-pipe should not rise above the centre line of the boiler, as when formed thus it gives better results than when carried higher. He also shows that the extension of the chimney downwards into the smoke-box offers material advantages.

E. R. D.

Ganz Steam-Motor Car, Erie Railway.

(Railway Age, Chicago, 5 July, 1907, pp. 12–13.)

This is the first of the type in the United States. It has four compartments, motor-, baggage-, smoking-, and general-passenger, being 58 feet long, weighing 45 tons and seating 50 people. The car is equipped with two compound enclosed steam-motors of 60 HP. each, mounted in the forward track, each driving one axle through gears. The cylinders are 4·7 inches and 6·7 inches, with 5½-inch stroke, the maximum tractive effort being 3,700 lbs., and the motors are so arranged that either may be operated independently of the other. All the working parts of the motors are inclosed in water- and dust-proof cases, and run in oil. The steam-generator, which is 42 inches in diameter and 5 feet high, is of 120-HP. capacity, with a heating-surface of 212 square feet and 6 square feet of grate-area. The steam-pressure is 270 lbs., the steam being superheated.

The speed is estimated at 40 miles per hour on the level and 15 on 1 in 50, these being reduced to 30 and 11 with a trailer. Particulars of bunker for coal, of which the consumption is estimated at 15 lbs. per mile, tank, brakes, lighting, etc., are given.

C. O. B.

Rail-Shifter. R. Budd.

(Engineering News, New York, 13 June, 1907, pp. 635–7.)

The article entitled "The Rebuilding and Double-Tracking of the Panama Railroad" is chiefly remarkable for the description of a machine for sluing rails. This consists of a double-drum hoisting-engine with a horizontal boom, and, working above it, a vertical boom. The rails are slightly raised by the latter by hooks on a sling chain, and shifted laterally by a hook and wire cable attached to the former. Details and illustrations are supplied.

In practice the best results are obtained by making two hitches to each rail-length of line, viz., about 15 feet at each throw, the travel in this case being about 4 feet at a time. The machine backs away from the rails as they are thrown, thus standing always on the old alignment, and it is given additional runs if the alteration exceeds about 4 feet. The machine is also found useful where merely lifting is required. The number of the crew and their wages are given; shifting 6,000 feet laterally 4 feet in 8 hours, resulting in a figure of $\frac{1}{4}d.$ per lineal foot, this being compared with $4\frac{1}{4}d.$, which would be the cost if done by ordinary methods.

C. O. B.

Preservation of Railway-Sleepers.

(Le Génie Civil, Paris, vol. l, pp. 444–5.)

The injection of antiseptics has been employed for many years for the preservation of timber, but the variety of methods used for impregnating with the same substance, for example, creosote, would alone suggest that the methods are purely empirical. With a view to ascertaining the best process for the preservation of sleepers the French Government have instructed Messrs. Devaux and Bouygues to go into the question systematically. The experiments are as yet by no means completed, but the results already obtained are of the greatest interest.

As it is more than probable that the decay arises from the action of organisms contained in the sap, the first point to be determined was whether the timber throughout its thickness was rendered sterile by the ordinary processes. That the antiseptic itself does not penetrate far into the timber is well known, and the first experiments were designed with a view to showing whether the heat, to which the material is exposed during impregnation, penetrates sufficiently to destroy any organisms which may exist near the centre of the sample. For this purpose rods of timber 15·75 inches in length were accurately turned to diameters varying between 1·89 inch and 4·92 inches, and were provided with an axial hole for a thermometer; these were encased, the ends being left open to the air, and the remainder of the sample was surrounded with an

atmosphere of either (a) steam at 100° C., (b) superheated steam at a temperature of 130° C. to 170° C., or (c) dry air at 140° C. It was found that the rate of transfer of heat from the outside to the centre was practically constant for each sample when the temperature at the centre was between 35° and 55°, and that this rate of transfer varied in the different samples, practically, as the square of the diameter, a result which coincides remarkably well with that obtained by Fourier when experimenting on homogeneous bodies. When steam was used as the surrounding medium, even when it was superheated, the sample showed a marked increase in weight. With dry air the rate of transfer was much slower than with steam. Experiments were conducted with full-sized sleepers when it was found that after exposure to the action of steam at 110° C. for one hour the temperature at the centre was in no case above 45° C., while at a distance of 1·57 inch to 1·97 inch from the outside the record was only 5° higher. With the system of impregnation used in the Government yards, the maximum temperature at the centre of the sleeper ranged between 46° and 55° according as heartwood or sapwood predominated in the sample. These temperatures are quite insufficient for the purpose of sterilization. I. C. B.

Tipping-Platform for Railway Trucks.

(Annalen für Gewerbe und Bauwesen, Berlin, 1 August, 1907, pp. 53-4.)

The great increase in traffic in recent years has revealed the existence of a serious shortage in railway-trucks, especially in the Rhine Provinces and Westphalia. Thus, in March 1907, out of 613,016 trucks requisitioned by the various works, no less than 53,696, or about 9 per cent., could not be supplied, which involved the detention at the collieries of some half-million tons of coal. Among the causes for the defective supply, it was alleged that there was frequently great delay at the stations and sidings, owing to the imperfect arrangements for discharging freight, and it was urged that better provision was needed for the mechanical emptying of trucks. An improved plan of rapidly and inexpensively emptying truck-loads of minerals, coal, etc., which is here illustrated and described, has been introduced by the United Machine Factories of Augsburg and Nürnberg. It consists of a platform hinged at one extremity and capable of being raised to an angle of 45° by an electric motor placed at the other end beneath the rail-level. The truck to be emptied is run on to the platform and firmly secured by double hooks attached to the front axle. The motor of 40 HP. is then started, and this raises the platform with its load by means of a screw which draws together the legs of two levers hinged at the upper end to the centre of the platform. The truck is then emptied at the front or narrow end and the contents are shot out into a bin, or down an incline. It is possible to arrange the plant so that, if

necessary, the loaded platform can be slued round at right angles to the line of rails. It will be evident that this system of mechanical unloading secures a great saving of time when large quantities of raw material have to be handled. It would involve, however, an alteration in the construction of the end of the railway-truck.

G. R. R.

Specifications for Reinforced-Concrete Structures.

(Report of the Committee on Reinforced Concrete of the Engineers' Club of St. Louis.)
(Journal of the Association of Engineering Societies, vol. xxxix, pp. 152–68.)

This report, which is now embodied in the building ordinances of the city of St. Louis, lays down rules for the design of reinforced-concrete structures. The Portland cement to be used is to be in accordance with the standard specifications adopted in June, 1904, by the American Society for Testing Materials, and the aggregate may be sand, broken stone, gravel, or hard burned clay. A fine aggregate is defined as one which will pass a No.-8 sieve, while coarse aggregate includes all sizes passing a 1-inch ring and caught on a No.-8 sieve. After certain clauses defining the grading of aggregates, the strength of the concrete is specified. For 28 days, burnt-clay concrete shall have an ultimate compressive strength of 1,000 lbs. per square inch, and all other concrete 2,000 lbs. per square inch. The steel is not to have a lower elastic limit than 30,000 lbs. per square inch, if of medium quality, and 50,000 lbs. per square inch for high elastic-limit steel. Methods of mixing the concrete and placing the steel accurately in the moulds are recommended, and the removal of forms in less than four days is forbidden, if, during the hardening period, the temperature is greater than 70° F. The assumed weights of various kinds of building material, and the allowances for live-loads are then specified. The unit-stresses recommended are as follows:—

Burnt Clay Concrete—		
Direct compression	300 lbs. per sq. in.	
Cross bending	400 ,, ,, ,, ,,	
Direct shearing	150 ,, ,, ,, ,,	
Shearing where secondary tension is allowed	15 ,, ,, ,, ,,	
For all other Concretes—		
Direct compression	500 ,, ,, ,, ,,	
Cross bending	800 ,, ,, ,, ,,	
Direct shearing	300 ,, ,, ,, ,,	
Shearing where secondary tension is allowed	25 ,, ,, ,, ,,	
	Medium Steel.	High Elastic-Limit Steel.
Tension	14,000	20,000

The bending-stress allowed for medium steel is 50 lbs. per superficial square inch of contact, and 30 lbs. for high elastic-limit steel.

R. S. B.

Reinforced-Concrete Towers. D. W. Krellwitz.

(Proceedings, American Society of Civil Engineers, vol. xxxiii, pp. 572–80.)

This Paper describes the design and construction of reinforced-concrete towers, 150 feet high, 11 inches square at the top and 31 inches square at the bottom, which were built for the Lincoln Light and Power Company on each side of the Welland Canal, in the Province of Ontario, for the purpose of supporting the wires conveying current from the transformer-building to St. Catherine's. These towers (said to be the highest monoliths in existence) are of square section throughout with chamfered corners, and each carries sixteen No.-1 bare copper wires on cross-arms 10 feet long and 3·25 by 4 inches section. Beneath the cross-arms there is a working-platform 10 by 5 feet, access to which is afforded by ladder-rungs cast into the concrete. The span over the canal is 76 feet, but adjacent to this there is a span of 300 feet. The towers are designed to withstand a wind-pressure of 30 lbs. per square foot on flat surfaces, and an ice-coating of 0·375 inch was allowed for on the wires. In calculating the sections three cases of stress were assumed:—(1) The tower without wires and not guyed, to withstand a pull of 2,000 lbs. at the top; (2) guyed tower, to withstand full wind-pressure and pulls transverse to the line, due to the wires; (3) assuming breakage of all line-wires per 300 feet span—the total pull to be taken on the guyed tower with full wind-pressure on the tower. The ratio of the moduli for steel and concrete was taken to be 15, and the tensile strength of the latter was neglected. The extreme fibre-stresses in compression were respectively 16,000 and 600 lbs. per square inch, and the allowable compressive stress in the steel was put at 6,350 lbs. per square inch. The combined area of the four steel reinforcing-rods was 19·6 square inches.

The base of each tower consisted of a cube of concrete 10 feet on each side, constructed with an opening on one side so that the tower could be raised from a horizontal position to the vertical without sliding horizontally. The towers were moulded in a position slightly inclined to the horizontal, and the concrete was composed of 1 part Portland cement and 5 parts gravel with sand, of which 36 per cent. passed through a sieve of 0·2 inch mesh. After hardening for 38 days they were erected by means of wooden shear-legs. During a test of one of the towers in position, a deflection of 2 feet was observed at the top, which did not cause any injury to the structure.

R. S. B.

Angle-Framed Retaining-Walls in Reinforced Concrete.

(Deutsche Bauzeitung, Berlin, 24 July, 1907. Supplement, pp. 53–4.)

The Author describes the general principles of constructing angle-framed retaining-walls in reinforced concrete and gives details of the actual construction of such walls, one of which was 70 feet in

length and 21 feet in height. The foundation conditions were very unfavourable and the permissible load on the footings was only 0·9 ton per square foot. Under these circumstances the horizontal leg of the angle had to have a length of nearly 15 feet. The wall was constructed of concrete consisting of 1 cement, 3 sand and 5 fine gravel, and was afterwards faced with granite masonry. Before use the reinforcing-bars were specially coated with fine rich cement-mortar, and the walls were backed after construction with a thick layer of asphalt. The Author recommends, in the construction of all reinforced-concrete retaining-walls of great length, the provision of an expansion-joint every 30–50 feet, as an absolute requirement for the safety of the structure.

The article is illustrated by various types of walls constructed on this principle.

F. R. D.

Lime Mortars. Prof. M. Gary.

(Mitteilungen aus dem Königlichen Materialprüfungsamt, Grosslichterfelde, Berlin, 1907, vol. xxv, p. 11.)

No mortar was used by the ancient Egyptians and Greeks in the construction of their buildings, reliance being placed on the weights of the massive stones which they used and the perfection with which their surfaces were prepared and fitted together. At a later period, when bricks had been introduced into Egypt, clay was used as a mortar, especially with unbaked bricks, and it is still used as a mortar in some countries. Nothing definite is known about the invention of lime mortar, but it is believed that burnt lime was at first used for the production of artificial stoves, Pliny mentioning that the column of Peristyl (3600 B.C.), in the Egyptian labyrinth, was made of this material. The aqueduct of Argos was also made of lime concrete. After its introduction into Greece, lime mortar was largely used in buildings, but hydraulic cement also soon came into use. The Romans obtained their knowledge of mortars and cements about 600 B.C. They introduced the use of cement into Germany, largely using the trass of the Eifel mountains instead of their Puzzuolana rock. During the middle ages the knowledge of the use of cements was lost, but lime mortars were still used; plenty of time was allowed for mortars to set, and they served their purpose even in thick walls, but in later days buildings were erected more quickly, and it was found when the Berlin Cathedral and the Dresden Kreuz church were pulled down that the mortar in the walls, which were 6 feet thick, although 140 years old, had never set, and was quite soft. It is now known that mortar will harden only in the presence of moisture and carbonic acid, which latter gas could not penetrate thick walls.

In former days much time and trouble was devoted to the preparation of mortar. A pit was dug on the building-ground, in which lime was slaked and allowed to mature slowly, and was then

mixed with sand by the master-masons who used it. Nowadays the practice in Berlin is to have the mortar prepared at works, but as about 1,500,000 cubic metres are used every year, and as the total capacity of the lime-pits is only 10,000 cubic metres, it is evident that very little time is allowed for the lime to be properly slaked. When the mortar, which should consist of 1 part of lime to 2½ to 3 parts of sand, arrives at its destination, it very often receives a further addition of sand, partly in order to save money and partly in order to reduce the shrinkage to a minimum, for with good mortar, quickly-built walls will settle down about 1 per cent. of their height. The Author suggests that, as each cartload of mortar contains about 6 cwt. of water, for which carriage has to be paid, it would be cheaper to adopt the American plan, and to supply the builders with dry mortar, consisting of sand and dry, slaked lime-powder. The Author has also carried out some experiments, of which the results are tabulated in the Report. He gives the crushing and tensile strengths of mortars containing various proportions of sand, and also of cement, for it appears that a slight admixture of the latter substance, say, 15 per cent., which would add only 27 per cent. to the cost of the mortar, increases its crushing strength by about 233 per cent., and materially accelerates its setting. Lime-mortar has practically no shearing strength, and should not be used where these stresses are likely to occur.

C. E. S.

Government Irrigation Project at Roosevelt Dam, Salt River, Arizona. Prof. Oscar C. S. Carter.

(Journal of the Franklin Institute, Philadelphia, 1907, pp. 277–310.)

Arizona has two distinct physiographical divisions; one is a plateau 4,000 to 8,000 feet above sea-level, the other a low-lying desert-area; and it is this area which the United States Government have undertaken to irrigate by damming up the waters of the Salt River by means of the Roosevelt dam which is now under construction. When completed the Salt River and the Tonto Creek will be banked up for 16 miles and a lake 25 miles long and 1 to 2 miles broad will be formed. By this means it is hoped to irrigate 160,000 acres. The soil of the desert-area is extremely fertile and only requires water; for instance, it will produce five crops of alfalfa grass per year if watered. The dam will be 285 feet high, 210 feet long, and will be a circle on plan convex up-stream, the back having a radius of 400 feet. It will be a masonry dam and the site is especially favourable, the foundations being solid rock with strata inclined about 30° dipping towards the reservoir. All the materials are on the site. The rock consists of a hard tough and fine-grained sandstone (crushing strength 1,000 tons per square foot), and provision has been made to transport blocks of stone up to 12 tons

in weight. Sand for mortar can be obtained from the river-bed, but has to be washed; it is therefore proposed to crush quartzite, which is found close to the site, hydro-electric power being used for the purpose. The materials for making Portland cement are also available. Important cement-works are therefore being established, as this will be cheaper than paying $9 per barrel for the 220,000 barrels required. Hydro-electric power will be employed for these works, but a difficulty arose in respect of the fuel for burning the cement-clinker; coal was out of the question owing to the expense, and wood, of which there is plenty, is not suitable, unless made into charcoal. It was finally decided to burn the cement with crude California petroleum: 11 gallons are required to burn a barrel of cement. The tensile strength of experimental cement made with the materials found on the site gave 775 lbs. per square inch after 28 days in water. An abstract of the specification for the construction of the dam is given, and the following are some of the points mentioned:—Explosives shall not be used in excavating the rock to avoid fracture of the rock forming the foundation; the Portland-cement mortar used in the lower 30 feet of the dam shall consist of 1 part Portland cement and 2 parts sand; and for parts of the dam more than 20 feet from the up-stream face, and 30 feet from the base, the cement mortar shall be 1 part Portland and 3 parts sand. Contracts for the work were let in 1904, and on the 3rd October, 1905, the excavation for and lining of the power-house canal and the work on the sluicing-tunnel were finished. It is not stated when it is expected that the dam will be completed.

H. R. S.

Gravel Catchpits for the Regulation of Mountain Streams.

J. Wey.

(Deutsche Bauzeitung, Berlin, 3 July, 1907, pp. 374–5.)

The Author describes in an illustrated article the gravel catchpits constructed for the regulation of the mountain streams in the Canton of St. Gallen, Switzerland. The object of these catchpits has been to prevent the heaping of gravel deltas in the valleys which were so often the cause of severe floodings in the lower reaches. He recommends as the best form an egg-shaped basin with a cross dam at the down-stream end, with an overflow-weir which should be raised to such a height as to prevent the gravel and stones from being carried over into the regulated channel of the stream below. In order that the depositing of the detritus may be evenly distributed over the whole catchpit a sluice-gate is provided in the dam, which is opened as soon as the water has commenced to flow over the dam. It is found that this means easily provides for all contingencies and facilitates the efficiency of the catchpit. The size of the pits is a matter of careful consideration, based on the watershed-area.

Some of these catchpits have been working over 3 years, and those with sluices have shown the advantages of the even distribution of the gravel over the bottom of the pit. The heavy material caught has been used to make the roads, the sand has been used for building purposes, and the fine mud for land-cultivation.

F. R. D.

Colorado River Diversion into the Salton Sink.

J. A. OCKERSON.

(Journal of the Association of Engineering Societies, vol. xxxviii, pp. 261–71.)

Many centuries ago the Gulf of California extended 150 miles farther north than at present, and the mouth of the Colorado River was then 120 miles from the north end. Eventually the sediment carried down by the river formed a delta which cut off the northern end, thus forming the Salton Sink, now called the Imperial Valley. This delta ranges in height between about 130 feet above sea-level at the United States–Mexican boundary line and about 20 feet on the western side of the gulf-basin. The waters in the northern portion of the gulf gradually evaporated, and the basin, with an area of 4,000 square miles, became dry and smooth, so that the Southern Pacific Railway follows it for a distance of about 75 miles. Various attempts were made to take water from the Colorado River to irrigate this arid area, and these culminated in the excavation of three intakes, the largest of which, 50 feet wide by 6 feet deep, was completed in October, 1904. As controlling-works or gates were not provided, the flood of 1904 tore out a deep channel, and, through the New River, the water entered Salton Sea. An attempt was made to stem the torrent by closing the channel with two rows of piles between which willows and sand-bags were placed; but this dam was quickly destroyed, as were also several other structures which were tried. Eventually a gate 200 feet wide, known as the Rockwood gate, was constructed, and was supported by some five hundred piles, and three lines of sheet-piling. Forty openings 4 feet wide were provided. This work was stopped by a flood, and the erosion of the southern bank left the headgate some 1,500 feet inland. The next plan tried was a system of permanent head-gates, made of reinforced concrete, with twelve gates 12 feet wide, the capacity being estimated at 10,000 cubic feet per second. The Southern Pacific Railway was washed out as the Salton Sea rose 40 feet, notwithstanding the efforts made to check it; so the Company then undertook the work of closing the break by a fifth attempt. The flowing channel, 600 feet wide, was crossed by a rock-fill dam on a mattress foundation 100 feet wide and 18 inches thick, and vast quantities of material were used, more than 1,000 men working on the dam. This, however, proved unsatisfactory and the river again broke through, but was again arrested by the construc-

tion of three parallel rock-fill dams in the by-pass. The sixth attempt at closing was successful as far as it went, though a new break occurred at a point 1,200 feet south of the end of the dam. The Southern Pacific Railway suffered great loss, and 8,000 to 10,000 people in the valley suffered great distress. The seventh attempt was made under difficulties, as floods of 30,000 to 50,000 cubic feet per second had to be contended against, and on the 11th February, 1907, the river was again flowing through the old channel and the passage to Salton Sea was dry. The Author states that the water now in the Salton Sea will evaporate in 10 to 12 years.

R. S. B.

North Sea and Baltic Canal Widening.[1] Scholer.

(Deutsche Bauzeitung, Berlin, 24 August, 1907, pp. 480-4.)

The Author begins with a retrospect of the original canal which was commenced in 1887 and completed in 1895 at the total cost of £7,800,000. The canal is about 62 miles long and has the cross-section bottom width 72 feet, 20 feet above bottom 184 feet, with a banket varying between 8 and 31 feet in accordance with the ground, and 6·5 feet below the mean water-level of the canal. Eight passing-basins 820 feet long and with 200 feet bottom width, were provided 7·5 miles apart. In short the canal was designed for ships of 475 feet in length, 75 feet beam and 28 feet draught.

Owing to the increase of the size of merchant-vessels and warships, it has been decided to reconstruct the whole canal at an estimated cost of £11,000,000 within the next 7 or 8 years. The new works will consist of the widening and deepening of the cross-section and of the straightening of the course of the canal. The depth will be increased to 36·5 feet as against 29·5 feet, the bottom width from 72 feet to 145 feet, and the width at water-level to 220 feet. The cross-section will thus have been doubled, having a total area of 9,000 square feet. The banks will be faced with dry stone pitching and have slopes of 1 to 2·25. The passing-basins will be eleven in number. Four of the basins will allow the turning of vessels, and have a bottom width of 1,000 feet and a length of 3,600 feet; the others will have a bottom width of 410 feet and will be 2,000 feet long. The new locks will have a useful length of 1,080 feet and a lock-chamber width of 148 feet, and will be built as twin locks at Brunsbüttel and Hottenau. The railway-bridge crossings are to be partly reconstructed so as to allow a clear headway of 138 feet. The swing road-bridges will be kept, as it is possible to widen the cross-section. All the other locks and drainage-sluices will be removed and reconstructed.

The article is illustrated by a comparative cross-section of the new and old canal.

F. R. D.

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[1] See Minutes of Proceedings Inst. C.E., vol. cxxiii, pp. 415-558; cxxix, p. 418; cxl, p. 312; and cl, p. 482.

Harvey Lock and Canal. GERVAIS LOMBARD.

(Journal of the Association of Engineering Societies, vol. xxxviii, pp. 272–82.)

This canal, which, about 90 years ago, was dug from a point opposite New Orleans on the Mississippi River to deep water in Bayou, Barataria, 5 miles away, has since been greatly improved, and the Author sketches the improvements made from time to time. It was originally only 12 feet wide, and the grade of the bottom was only 3 feet below the mean level of the gulf. In 1858 it was dredged to a depth of 60 feet, and owing to the increasing trade it was decided to connect it directly with the Mississippi River by means of a lock constructed in 1882. This lock, however, failed soon after construction and was replaced by an earthen dike. In 1902 a contract was let to enlarge the canal for 2 miles nearest the river, to 80 feet and a depth of 7 feet below lowest canal-level. Test-borings were made at the site of the old lock, from which it was found that there was nothing but fine sand for a depth of 81 feet below the natural surface of the ground, notwithstanding which the site was chosen and piling was resorted to. Round piles, 45 feet long and not less than 15 inches in diameter at the butt end, were used, and they were spaced 4 feet apart. To the heads of these piles 12-inch by 12-inch caps were bolted transversely, and after the earth was excavated to a depth of 1 foot below the tops of the piles, concrete (1–3–5) was filled in, and was reinforced with corrugated-steel bars. Each gate is swung on a concrete monolith containing nearly 1,200 cubic yards; the chamber between the gates is 180 feet long and the retaining-walls consist of a triple-lap pile-bulkhead rising about 4 feet above mean canal-level and backed with earth. The gates are of steel, each being 17 feet 5 inches wide by 29 feet 4 inches in height and 15 inches thick, and are stiffened by 15-inch beams. Leakage is prevented by having the pintle of the gate reduced in diameter, so that the water-pressure would force the gate against the hollow quoin. A steel draw-bridge of 35-foot span with a 14-foot roadway crosses the canal. The lock was started in 1905 and the official tests by the Board of Commissioners for the Lafourche basin took place, but leakage began under the front gate, the water having penetrated beneath the foundation into the quicksand. The remedy suggested by the engineer-in-charge (and ultimately carried out) was the driving of interlocking sheet-piling, 38 feet long and 12 inches wide, weighing 40 lbs. to the square foot, while the concrete retaining-walls, joining the abutments of the new gate to the old, had to be removed by blasting. A steam-hammer weighing 10,000 lbs. was used for driving the piling, and great difficulties were experienced in driving. Cement grout was run under the floor of the lock through holes cut in the concrete and the void was thoroughly filled. These repairs proved to be successful.

R. S. B

Floating Dock No. IV at Rotterdam.

(Zeitschrift für Elektrotechnik und Maschinenbau, Potsdam, 1 May, 1907, pp. 187–92.)

The important shipping interests of Rotterdam being inadequately served by the three existing floating-docks, the Corporation decided to build a much larger one, capable of lifting ships of 18,000 tons displacement. The contract was let in May, 1902, to the firm of Aug. Klönne, of Dortmund, for £90,600, and this article gives a description of the new dock and of the method of constructing it. The overall dimensions are 556 feet long, 117 feet beam, and 49 feet high; the docking space is 86 feet wide at the bottom and 94 feet at the level of the top of the side-tanks, which are 36 feet high. These side-tanks are placed on seven pontoons each 117 feet long, 87 feet broad and 13 feet high at the centre, and 12 feet at the sides; a space of 2 feet is left between each pontoon. The pontoons are independent of each other, and are connected to the side-tanks by means of large screw-bolts; they can be detached and docked for repairs. Each pontoon is divided into four water-tight compartments and weighs 590 tons. The side-tanks have a deck 19·5 feet above the floor of the dock, extending the whole length and used for the pumping- and other machinery. The space under this deck is water-tight and is filled with water when the dock is lowered; the side-tanks have stiffening members placed 3 feet 3 inches apart and the total weight of both tanks is 1,770 tons. Details of the construction and the scantlings are not given in this article. The pumping arrangements consist of fourteen centrifugal pumps, each driven direct by an electric motor and having a capacity of 26·1 cubic yards per second. Two of these pumps deal with each pontoon, each having its own suction-pipe of 11·81 inches diameter. In building the dock a piece of ground was prepared near the water's edge by piling, and was connected to the railway by a branch so that the material could be brought on to the site in railway-wagons. Each pontoon was built separately and launched, and, when all were completed, they were coupled together and a line of railway laid over them and connected with the shore to transport the material for the side-tanks which were then built on the pontoons. Several photo-reproductions of the dock during construction are given.

H. R. S.

Submarine Bell-Signals.

(Archiv für Post und Telegraphie, Berlin, 1907, p. 443. 3 Figs.)

In this article the Author describes the submarine bell-signals which are now used in foggy weather on various lightships, and the method adopted for receiving such signals on the large ocean liners. The illustrations show the angle within which the sound-waves can be picked up by the receivers on the vessels. In 1905 the first apparatus for German use was made by the Nord-Deutscher Lloyd, and supplied to the lightships *Aussenweser*, *Elbe I* and *Gabelsflach*, and in 1906 to the *Aussenjade*, and a number of liners were fitted with microphone-receivers.

The signal-bell is hung about 20 feet deep in the water by a chain in the case of three of the lightships, but it seems better to have a shaft passing through the keel of the lightship and to let the bell down when required. The clapper is worked by hand, steam, compressed-air or electricity, and the signal consists of a certain number of strokes, the number being different for each lightship. The Author then gives details of the results obtained; for example, the *Deutschland* on its passage out heard the signal of the *Weser* lightship 15 miles away, and the throbbing of the propeller of another steamer could be heard when 1½ mile distant. The *Amerika* heard the signal of the *Weser* when 6¾ miles distant; the *Kaiser Wilhelm II*, in a thick fog on the 12th March, 1906, heard the submarine bell-signals with the starboard-receiver when 10 miles from the lightship, and therefore changed the vessel's course one point to starboard, picking up the signals with the port-receiver. The Author gives a considerable number of further reports, from which it appears that the apparatus is very satisfactory.

E. R. D.

Problem of Aviation and its Solution by means of the Aeroplane.

J. Armengaud.

(Bulletin de la Société d'Encouragement pour l'Industrie Nationale, Paris, July, 1907, pp. 847–73.)

Soon after the siege of Paris, where the balloons rendered such important services, the Author with his friends founded the French Society of Aerial Navigation, and the problem they were formed to study may now be considered as being solved, since it has been possible to devise a light and powerful motor. A brief review is given of the past history of aerostation, leading up to the successful experiments of Mr. Santos-Dumont. The Author divides the subject for consideration into two main branches—aerostation and aviation, and explains exactly what is intended under each definition. The present treatise is devoted to the latter branch. An account is given of the aeroplane of Mr. Santos-Dumont, with the various trials

ending in the flight of the 23rd October, 1906, by which he gained the Archdeacon cup, and the still more successful experiments on the 12th November, 1906. A succinct history is given of previous attempts at aviation from the time of Icarus onwards. The Author dwells on the researches of the Vicomte de Ponton d'Amécourt, and explains his model apparatus. The three systems of appliances for flight may be described as orthoptera, helicoptera and aeroplanes. The first rely, as do the birds, on the stroke of the wings or blades on the air; the second series rely on the motion of the screw to obtain ascensional power, and the third give preference to the use of gliding planes for sustension. An outline is given of the labours of Messrs. Pénaud and Hureau de Villeneuve, who obtained a prize from the Académie des Sciences for their treatise on the theory of flight, and the attempts made at Lilienthal and by the brothers Wright at Dayton, Ohio, are explained. The relation of the aeroplane to the kite is discussed, and it is shown how the theory of the aeroplane has been adapted to practical trials by Mr. Santos-Dumont and others. Special reference is made to the work of Colonel Renard, and to his deductions as utilized in the Santos-Dumont apparatus, also to the investigations of Colonel Vallier and his conclusions respecting the dynamics of the aeroplane. The various experimenters who are now engaged in attempting the solution of the problem in various ways are enumerated, such as Messrs. Kapférer, Vuia, Delagrange, and Esnault-Pelterie. The Author states that Mr. Levavasseur and Captain Ferber have under construction some aeroplanes to carry two persons, furnished with a motor of 100 HP. Commander Paul Renard stated that he was persuaded that the present century would witness the practical realization of human flight.

G. R. R.

Dimensions of Petrol Motor-Car Engines. HERMAN WILDA.

(Deutsche Techniker Zeitung, Berlin, May, 1907, pp. 226–8, and 242–3.)

In this series of articles a complete set of formulas are worked out by means of which the leading dimensions of small petrol-engines can be calculated. These formulas are derived by reasoning which is partly theoretical and partly practical, that is, the method employed is to establish a theoretical formula including such constants and variables as are appropriate to each case, and then determining the constants by plotting the results obtained by applying the formula to the dimensions of actual petrol-engines which have given satisfaction. A complete summary of all the formulas is given, and a numerical example is worked out for an engine suitable for a commercial motor-car weighing 2,000 kilograms (say 2 tons), and having a maximum speed of 10 kilometres (6·2 miles) per hour. It is considered that an engine developing 3 to 3½ B.HP. at 1,200 revolutions per minute would be suitable, which is taken to require

7 I.HP. The formulas are divided into five groups, namely, those for the cylinders, for the crank-shaft, for the connecting-rod, for the fly-wheel, and for the valves and pipe-work. The majority of the formulas are made dependent on the diameter of the cylinder, and this is calculated from the following:—

$$D = \sqrt[3]{\frac{N_e \times 2,290,000}{\frac{S}{D} n \left(4 \cdot 8 + \frac{1}{m}\right)}}$$

where N_e is the indicated horse-power, S is the stroke, $\frac{S}{D}$ is "assumed" and in the numerical example is taken as 1·125, n denotes the revolutions per minute, m is the ratio of the volume swept by the piston to the clearance space (taken at 15 per cent. in the numerical example). D works out to 80 millimetres (3 inches) and S to 90 millimetres (3½ inches) in the numerical example given.

Altogether thirty-eight formulas are given, some of which are as follows:—

$$\text{Thickness of cylinder-walls} = 0 \cdot 0625\ D.$$

$$\text{,, \quad ,, piston-rings} = \frac{D}{28}$$

$$\text{Weight of the reciprocating parts} = 0 \cdot 039 D^2 \frac{\pi}{4} \text{ kilograms.}$$

$$\text{Area of exhaust-pipe} = \frac{D^2\, S\, n}{270,000} \text{ square centimetres.}$$

H. R. S.

New Method of Promoting Circulation in Boilers.

(Le Génie Civil, Paris, vol. li, pp. 129–30.)

The formation of scale in a boiler and the consequent rapid deterioration of such plates as are exposed to the direct action of the fire, is usually caused by defective circulation, which also lowers the evaporative efficiency in a marked degree, thus, on all grounds, activity of circulation is to be sought after. Generally, such circulation is brought about merely by the direct ascent of the bubbles of steam, but this action is very local, and, owing to the considerable velocity which the bubbles have acquired before reaching the surface, they cause particles of water to be projected into the steam, thus augmenting any tendency towards priming. The apparatus under review can be added to any boiler in which the heating takes place from within tubes or flues, one of the simplest cases being that of a Lancashire boiler. The arrangement consists, essentially, of one or more shields each in the form of a very shallow inverted trough, fixed above the flue or tube from the outer surface of which the steam

will be generated. An aperture is left in the crown of the shield which is surmounted by a box, wedge-shaped in plan, and closed on the top and the two sides, the bottom being formed by the shield, and the aperture in the latter serving to admit the steam, collected by the shield, into the box, which, together with the shield, is totally submerged when the water in the boiler is at its normal level. On the steam reaching this box it escapes, with comparatively small velocity, through guiding pipes leading in a more or less axial direction as regards the boiler, from the blunt end of the box, and in doing so it puts in motion the adjacent water without causing serious eddies. A diagram is given in the text showing, during a day's work, the respective temperatures above and below the fire-box and also that corresponding to the steam-pressure. Before the steam began to be drawn off the boiler behaved exactly as it would have done without the device in question, but no sooner was the valve opened and the steam allowed to escape, thus relieving the pressure and permitting fresh steam to be generated, than a marked difference was noticed, and the excellence of the circulation was proved from the fact that the temperature below the fire-box rapidly rose to within a few degrees of that corresponding to the steam-pressure, this temperature being maintained so long as steam was being generated.

It is claimed that 7 to 8 per cent. of fuel is saved by the adoption of this method of circulation.

I. C. B.

Use of Steam at very High Pressure. Léon Creux.

(Le Génie Civil, Paris, vol. li, pp. 37–39.)

It has been shown experimentally that steam, at a temperature of 500° to 600° and with a consequent pressure of as high as 1,422 lbs. per square inch, can be used for generating power, but in order to do this two important difficulties have to be overcome:—(1) that of generating the steam; and (2) that of lubricating the cylinder-walls. In the apparatus under notice the boiler or generator is of the flash type and is composed of a number of elements, each consisting of four parallel tubes placed one above the other and coupled together by bends in such a manner as to form one continuous length. The lower end of each element is coupled to a feed-water pipe which is common to all, and the upper end to a steam-receiver which is also common. The bottom pipes, one for each element, form the crown of the furnace and they are kept at a dull-red heat. The feed-water, having previously passed through an economizer, is forced into the bottom tubes at a pressure of 28 to 56 lbs. per square inch above that of the steam, through a tortuous orifice which causes it to be projected along the hot generating-tube in the form of spray, a condition in which, owing to its finely divided state, it will very readily absorb heat. The remaining three tubes of each element are

more of the nature of superheaters, the true vaporization having been almost completely effected in the bottom tube. The fact that all the elements are "in parallel" is advantageous from many points of view, not the least being the facility with which any element can be isolated. The steam-pressure is regulated by that in the feed-pipe, as, should it rise above this, no water can flow into the heated tube and thus no further steam can be generated until the pressure again falls. Turning to the second difficulty, viz., that of lubrication, it is found that if the oil be kept from contact with either air or steam it retains its lubricating properties even when heated to the very high temperature in question. In order to keep the oil thus sealed up, annular grooves are cut in the piston. The oil in the simplest case, i.e., a single-acting cylinder, is forced into a groove near the end of the piston on which the pressure acts, and as the piston reciprocates, it travels particle by particle, towards a similar groove near the other end, whence it is drawn off for re-use. A table is given showing the saving of a machine working at 426 lbs. per square inch as compared with one of similar power working at one-third of this pressure; this is computed as 25 to 29 per cent. It is pointed out that the diminished weight per horse-power of the high-pressure machines is of especial moment where questions of transport over rough country arise.

I. C. B.

New Appliances for Continuous Brakes. A. Boyer-Guillon.

(Le Génie Civil, Paris, vol. li, pp. 213-6.)

Although the Westinghouse brake is perfectly satisfactory for velocities not exceeding 56 to 62 miles per hour, with the present ever-increasing speeds a more efficient appliance becomes year by year more necessary. With regard to the general action of brakes, it has long been known, *inter alia*, (1) that the instant before the wheel begins to skid the retarding action of the brake suddenly increases to its maximum; (2) that when the skidding has once begun it will continue until the pressure on the brake-block is relieved to a very considerable extent, i.e., the retarding effect is greatly reduced as soon as skidding begins; (3) that the friction between brake-block and wheel increases as the peripheral speed of the latter diminishes. Thus it would follow that if, to the wheels of a train in quick motion, the brake-blocks be applied with a pressure approaching that which would cause the wheels to skid, as the velocity decreased the increased friction between block and wheel would soon cause the latter to skid and the efficient action of the brake would thereupon cease. With the appliance under notice the force with which the block is applied to the wheel is regulated by the frictional effect it produces. The two brake-blocks on each wheel react the one upon the other, so if the pressure on one is eased that on the other is similarly affected. One of these blocks, instead

of being hung from the framing of the wagon, is attached to a lever, which, in turn, is so coupled to a helical spring that the latter is compressed if the lever be moved either way from its normal position. On the brakes being applied the block in question tends to rotate with the wheel and is prevented from doing so by the spring, which latter begins to deflect as soon as the force opposed to it has attained a predetermined magnitude. As soon as this deflection takes place a pawl is allowed to engage with a rack attached to the connecting-rod from the brake-cylinder, and while this is engaged no further pressure can be brought to bear on the brake-blocks. As the speed of the train is reduced the deflection of the helical spring would tend to increase by reason of the increased friction between the block and the wheel, but through the intermediary of a cam any further movement of the block allows it to increase its distance from the centre of the wheel, and thus the pressure is being relieved as the friction between block and wheel increases by reason of the diminishing speed of the latter. For light application of brakes the arrangement described does not come into play, but when a sudden stoppage is required it enables the maximum brake-pressure to be applied without fear of skidding. The fact of the sudden increase in frictional resistance immediately before the wheels begin to skid makes it practicable to adjust the spring so that the blocks will be applied with greater force than would otherwise be permissible.

I. C. B.

Mechanical Concrete-Mixer. R. Bonnin.

(La Nature, Paris, 3 August, 1907, pp. 145-7.)

On certain of the works for the Paris Metropolitan Railway the concrete is being mixed in a machine, electrically driven by a dynamo of 20 HP., which does its work in a very satisfactory manner. It consists of a metal frame, carrying the dynamo and the mixing-drum, which is a horizontal cylinder provided with stirring arms; above the cylinder is a tank to hold water, and beneath it are two smaller measuring-tanks, each one adjusted to hold exactly the right proportion of water for each charge of the concrete-mixer. These smaller tanks are quite distinct from the upper main tank and are filled and emptied by a special arrangement of pipes and valves, the working of which is explained by reference to diagrams. A side lift is attached to the mixer by means of which the measured quantity of cement and aggregates can be drawn up to the top of the mixing-cylinder and discharged automatically. The aggregates are first stirred together dry by some 7 or 8 revolutions of the mixer, and then the measured volume of water is admitted by the man in charge, and the contents are thoroughly incorporated by 7 or 8 more turns of the cylinder. The cylinder or drum has a hole in the side,

and by means of a catch the workman can at any moment stop the revolution of the casing, which does not stop the stirring arm. As soon as the casing is blocked, a sliding door is opened and the concrete falls out into a truck placed beneath ready to receive it. The whole operation of filling, mixing and emptying occupies about $2\frac{1}{2}$ minutes, and, as the charge is 500 litres, it is possible to produce 11 to 12 cubic metres (386 cubic feet to 423 cubic feet) of concrete per hour. An analysis is given of the cost of the operation and it is stated that a cubic metre of well-mixed concrete can be prepared at a cost of 1 franc 25 cents (say 9*d*. per cubic yard).

G. R. R.

Blasting Rock in Large Masses. George C. McFarlane.

(Engineering and Mining Journal, New York, 3 August, 1907, p. 204.)

In excavating very hard rock, the cost of drilling the shot-holes is very high. The Author describes a method for reducing this cost to a minimum adopted on a section of the Grand Trunk Pacific Railway where the cutting is through extremely hard granite and tough trap. The method is that of deep holes and heavy charges to remove the rock in large masses. In the smaller cuts, hand-drilling is preferred, the depth of the shot-holes often being as great as 30 feet. For these 1-inch steel is used, and the same gauge—$1\frac{3}{8}$ inch—is maintained throughout. The holes are started with two hammers on a drill, and when down 5 or 6 feet the drill-turner also swings in with a hammer. The rapid blows jump the drill sufficiently to bore a fairly round hole. The average depths drilled per day by three men varied between 16 feet and 29 feet. Machine-drills are used in the big cuts, a 3-inch machine drilling to 25 feet and a $3\frac{1}{4}$-inch machine to 30 and 35 feet. In these the drill-steels are made up for 24-inch runs, the starter being gauged $3\frac{1}{2}$ inches and the gauge reduced by about $\frac{1}{8}$ inch for each succeeding steel, so as to finish the hole at about $1\frac{1}{4}$ inch. The bits are forged with long heavy shoulders and very little clearance. The cheaper qualities of drill-steel are used almost exclusively, it having been found that even when properly dressed and tempered, the better qualities wear as fast as the lower grades. In tempering, it has been found that the bit should be toughened by heating to a bright red, then plunging into water $\frac{5}{8}$ to $\frac{3}{4}$ inch and holding it there for 15 to 20 seconds, afterwards sousing a few times till the part out of the water shows no colour, and finally immersing in the tub till cold. When tempered in this way a drill will show $\frac{1}{2}$ inch of cutting-edge with a fine grey temper backed by softer tough metal.

After drilling, the bottom of the hole is chambered to the required capacity by "springing" or "squibbing" with strong dynamite. In the bottom bench, where there is a heavy lift, only about a foot of the hole is chambered, but in the higher benches it

may be chambered between 2 and 3 feet from the bottom. For this work water-tamping is used. After each springing shot, the hole is blown out with steam or pumped with a sludge-pump. The chambering shots are repeated till the pocket has been made large enough to hold the blasting charge. The following are typical chambering and blasting charges:—(1) A 25-foot hole, burdened 18 feet, in the bottom bench of a 45-inch cut. The chambering shots were, first shot, two sticks (cartridges) of 60 per cent. dynamite; second shot, 4 sticks; third shot, 10 sticks; fourth shot, 25 sticks; fifth shot, 60 sticks; sixth shot, 100 sticks; seventh shot, 180 sticks; blasting charge, 325 lbs. of black powder: (2) A 25-foot hole, burdened 12 feet, in the upper bench of a 45-foot cut. First chambering shot, 6 sticks; second shot, 20 sticks; third shot, 60 sticks; fourth shot, 125 sticks; blasting charge, 325 sticks, 150 lbs. of 40 per cent. dynamite. The most economical shots are from holes 16 to 24 feet deep and burdened between 12 and 15 feet. The bulk of the rock is heaved out 20 to 50 feet, and fragments are rarely thrown farther than 150 feet.

G. G. A.

Air-Hammer Drill at Work. G. E. Wolcott.

(Engineering and Mining Journal, New York, 20 July, 1907, p. 117.)

The air-hammer rock-drill is everywhere displacing the piston-machines in all but the heavier classes of work. For sinking it is not very suitable, and it has not yet demonstrated its superiority over the piston-machine in drifting. But for stoping in mines, and for all overhead work, its suitability has been proved and its superiority is now generally acknowledged. For stoping-work especially, it is coming rapidly into favour on account of the ease with which it may be handled. In starting to drill, all that is necessary is to lay a short plank on the rock debris—"muck-pile"—and to place the point of the air-feed on it. Even the plank, though an advantage, is not necessary. In practice it is easy to begin drilling in less than 15 minutes after entering the stope. With the air-hammer drill the holes may be placed where wanted and in the desired direction, while with the piston-drill, using a bar, the holes must radiate from the bar as a centre, and their number is limited by the reach from the set-up. At the Findlay mine of the Consolidated Gold Company, at Cripple Creek, Colorado, the 2½-inch piston-drills will not average over 25 feet of drilling in the 8 hours shift, and the depth of holes is seldom over 5 feet. With the air-hammer drill the average number of feet drilled is 40, and the holes will average over 5 feet in depth, 6- and 8-foot holes being not uncommon, with a bottom diameter of a little over 1 inch. The consumption of air per shift is about the same as for the piston-machine. The value of the air-drill does not lie in the fact that it

will bore faster than the piston-machine, but in being more simply and lightly constructed—which means a lower first cost and less for repairs—and in requiring less time for setting up, taking down, changing steel, etc. Dust is a somewhat serious objection to this type of drill when hollow steel is used. But in most circumstances hollow steel is not necessary for overhead stoping. The drill used at the Findlay mine is a radical departure from that commonly used with machines of this type. Usually the six-point bit is preferred with hollow steel, and either the six-point or the cross bit with solid steel. At the Findlay mine the form adopted is the single bit having a blunt point (about 90°). In practice this has given great satisfaction. It is possible to give the bits a very hard temper without risk of breakage. While "filchering" better than the six-point bit, it will pass seams much more readily than the piston-machine, and a hole may be started on a sloping face with little difficulty.

G. G. A.

Tests on Wet Emery- and Carborundum-Wheels.

G. Schlesinger.

(Zeitschrift des Vereines deutscher Ingenieure, Berlin, 3 August, 1907, pp. 1227-30.)

These tests were carried out in order to show that the peripheral speed of 82 feet per second, laid down as the maximum permissible by the Prussian Ministry of Commerce in 1897, was no longer justified in view of the present excellence of emery- and carborundum-wheels due to improvements in design and manufacture. The tests were carried out under workshop conditions on cast- and wrought-iron specimens on a standard grinding-machine. Wheels from stock $19\frac{5}{8}$ inches in diameter and about 2 inches wide were tested to about five times their normal capacity, a stream of water passing over them while under test. Destruction-tests were also attempted, in which it was found that disintegration took place rather than bursting, the emery in one case actually peeling off for a depth of about 2 inches in less than three minutes. The Author came to the conclusion that it should be obligatory for manufacturers to specify clearly on every wheel sold (*a*) whether the wheel is to be used wet, dry, or both; (*b*) that the wheel has been tested to a maximum number of revolutions corresponding to a peripheral speed of at least 164 feet per second; (*c*) the maximum number of revolutions at which the wheel may be run, this being such that when the wheel is new the peripheral speed is about 115 feet per second.

The article is accompanied by a number of illustrations and diagrams.

C. J. G.

Large Gas-Engines.

(Engineering Record, New York, 24 August, 1907, pp. 51–2, supplement.)

The largest gas-engines in the States as regards cylinder-diameter, 44 inches by 54 inches stroke, are described and illustrated. On blast-furnace gas of about 80 to 85 B.Th.U. they will give 4,000 HP. and up to 5,000 HP. on richer gases. The valve-gear is located between the engines concentrating the gear on a twin tandem in a way which makes it convenient for the driver. The engine is of the cut-off type, the quantity of gas and the time of admission both being under the control of the regulator. The weight is in round numbers 1,500,000 lbs.; the crank-pins are 20 inches in diameter; the shaft is 30 inches in diameter in the bearing; and the fly-wheel is 23 feet in diameter, weighing 200,000 lbs. The speed of the engine is 83⅓ revolutions per minute. The pistons and rods are water-cooled, water being introduced at the centre and flowing forward to a discharge in the frame for the front piston, and backward to a discharge in the tail guide for the rear piston, each having its separate supply. Thirty-six of these are being built by the Allis-Chalmers Company, Milwaukee, for the power-house of the Indiana Steel Company's new works at Gary, Indiana, and for other large installations.

C. O. B.

Gas-Producer using Pulverized Fuel. F. D.

(Le Génie Civil, Paris, vol. li, pp. 22–4.)

The quantity of fuel required for the production of a given horse-power when using an internal-combustion engine is small in comparison with that required when the power is obtained through the intermediary of steam, but this gain is greatly discounted by the higher price of the fuel suitable for the former. In the producer under review a much cheaper fuel can be used than has hitherto been employed; indeed, from the fact that before use it is reduced to a fine powder, coal which is broken into too small pieces for ordinary use, and is therefore almost unsaleable, is a perfectly efficient fuel. In addition to this it is claimed that all classes of coal or coke can be used whether they contain much or little volatile matter, and further that fuel with a percentage of ash as high as 70 can be employed. This latter is a very important point, as for ordinary steam-raising purposes the value of a coal diminishes with great rapidity as the percentage of ash increases, for instance, a coal with 35 per cent. ash has not half the commercial value of one with only 20 per cent. ash.

The producer consists of a receiver, circular in plan, lined with some refractory substance. Near the bottom of this and almost tangential to the circumference of the lining is inserted the discharge-pipe of a blowing-fan by means of which the pulverized

fuel, together with the requisite quantity of air, is introduced. The producer is put in action by lighting a wood-fire inside and subsequently blowing in the coal-dust by means of the fan. A very high temperature is produced, the residue of the fuel is melted and is thrown by the centrifugal action of the air-jet on to the vertical sides of the lining, whence it falls in small drops into a cavity formed in the bottom. Every 6 or 8 hours the slag so collected is drawn off and this is the only cleaning required. Owing to the exceedingly high temperature any tar which may be formed is immediately converted into a true gas, and when one of the producers was opened out after having been at work for 18 months no trace of this substance could be found. The largest plant at present at work is capable of giving out 600 HP., this being much higher than can conveniently be obtained from any single producer using fuel of the ordinary kind.

I. C. B.

Hydraulic and Electric Cranes Compared. R. Gasquet.

(Le Génie Civil, Paris, vol. li, pp. 209–33.)

In comparing the relative values of hydraulic and electric cranes for the purpose of handling cargo many points have to be taken into account, among which may be named:—(1) the ease of working and the precision with which the load can be handled; (2) the length of preliminary training required before an average driver will become efficient; (3) the amount of noise during working; (4) the cost of working; (5) the initial outlay. Although by reason of the incompressibility of water it would seem to be a very simple matter to control accurately the load of a hydraulic crane, such is not the case, owing to the minute movements of the valves causing a great variation in the quantity of water permitted to pass, and it is found that a longer term of probation is required for drivers of hydraulic than for those of electric cranes. Noise is a matter of considerable importance when it is remembered that in discharging a ship all orders must be given orally by a man in full view of the hold. The hydraulic crane, owing to its slow movement, is particularly silent, but now that separate motors are used for lifting and sluing, while hide pinions are employed for the quick-running gear-wheels, a great improvement in this respect has already been effected in electric cranes.

As regards upkeep the slow motion of the hydraulic crane reduces this to a minimum, and even with motors running at speeds as low as 170 revolutions per minute the upkeep must be considerably above that of hydraulic cranes. Frost is, of course, an evil to which the hydraulic system alone is susceptible. Turning to the subject of efficiency the electric crane holds by far the higher place, as the power required is proportional to the load, whereas a

hydraulic machine, unless unduly complicated by additional rams, uses the same power for light load and heavy. The initial outlay may be divided under three heads:—(*a*) the cranes proper; (*b*) the feeders, i.e., pipes or cables; (*c*) the central station. The electric cranes themselves are slightly dearer than the hydraulic cranes, but the cables required for the electric supply are far cheaper than the pipes needed for the water under pressure, and considering that in most towns electric light is now in use, a central station can generally be dispensed with, the electric energy being taken from the lighting circuits, this being as a rule a convenient arrangement, as the cranes are chiefly in use during the day when the central station is running on a light load. The general conclusion is that for a new installation, electricity should be employed, but the advantages are insufficient to warrant the abandonment of existing hydraulic plant.

I. C. B.

Travelling-Crane with Folding Jib. HÖNSCH.

(Annalen für Gewerbe und Bauwesen, Berlin, 1 September, 1907, pp. 105–7.)

It is pointed out that one of the disadvantages of railway travelling-cranes for breakdown-gear is that their use in a train involves the employment of a spare truck in front, in order to protect the jib. If this jib is raised, it is too lofty to pass along the line, and if lowered for transport purposes it projects far beyond the buffers of the truck carrying the base-plate. By reference to a series of process photographs a description is given of the patented arrangement introduced by the Breslau Railway Carriage and Machine Building Company to overcome these difficulties. The jib is hinged in the centre, and the winding-gear is so contrived that it can be used either for lifting purposes or for lowering and raising the jib, but the free pulley carried by the jib for the lift can be pulled back and employed to double up the jib itself, and to cause it to bend down to the truck-level. This is effected simply by winding in the hoisting-chain, and the crane-jib can be doubled up ready for running over the rails in the space of three minutes. This improvement can be applied to existing machines, and cranes on this principle are being built to lift 5 to 7½ tons.

G. R. R.

Novel Derricks for Reinforced-Concrete Warehouse Erection, Chicago.

(Engineering News, New York, 25 July, 1907, pp. 81–2.)

There are four of these for the building, which is 750 feet long, 150 to 270 feet wide, and 135 feet high. Each derrick is 135 feet high and 10 feet square, of framed steel to a height of 85 feet, where

it is guyed at the heel of the steel-framed 85-foot boom. Above the 85-foot level the structure is reduced to 6 feet by 4 feet in plan. The area commanded is a complete 170-foot diameter circle, with a maximum hoist of 150 feet. The arrangements for the circular motion of the boom at its heel are fully explained. Each derrick stands within one proposed floor-panel, the reinforcing-rods projecting through from the adjacent panels being bent up out of the way till after the rest of the work is completed, and the derrick removed. The hoisting is done through the towers which form the lift- and stairway-shafts, the bottom of them being connected by portable railways with concrete mixers, etc. The guys are so arranged as not to foul any of the girder or column forms, and they pass through pipe-sleeves set in the floor-slabs as the work progresses.

As it was considered economical to lift the concrete in large masses in $1\frac{1}{2}$-cubic-yard buckets, while distribution in smaller quantities at various points was necessary, a special bucket to accomplish this was designed, of which a full description is given Each derrick has its 25-HP. electric hoisting-engine.

C. O. B.

Derricks for Building-Construction.

J. W. Koehler.

(Zeitschrift des Vereines deutscher Ingenieure, Berlin, 27 July, 1907, pp. 1189-91.)

The type of derrick described in this article is a cross between a mast-crane as used in America and the English pattern of derrick. It consists essentially of three parts, the guide-framing, the crane-tower, and the hoisting-gear. The guide-framing runs along the outside of the proposed building and is built up in panels of 32·8 to 42·7 feet, with brace-rods $\frac{13}{16}$ inch in diameter. A broad-flanged beam turned over on its side constitutes the top of the framework, and serves to guide the rollers on the crane-tower, while struts carried through future windows or doors in the buildings serve to stay up the framing. The crane-tower, which is about 4 feet square, is built up to any required height in structural steel and its foot rests on a roller carried by a single rail. To prevent the rollers from jumping out of the broad-flanged beam on account of inequalities in the rail, the roller-support is pivoted and provided with a safety-device, while a wire-rope tension arrangement with a safety-attachment preserves the erect position of the tower. The reach of the crane itself is 10·2 feet clear, and loads up to 3 tons may be safely lifted by a crane 82 feet high. By the use of an auxiliary rope-sheave loads up to 6 tons may be lifted at half the normal reach. The total weight of the crane itself is 3·6 tons, while 115 feet of guide-framing weigh 3·9 tons. The weight to be carried from building to building is therefore only 7·5 tons, exclusive of the

weight of the hoisting-gear. A width of 9·84 feet is required by the crane in front of the building, and trees, lanterns, or tramway-poles need not be removed, as the crane clears them.

The article is accompanied by a number of illustrations and diagrams.

C. J. G.

Ship-lift at Romanshorn, Switzerland.

(Schweizerische Bauzeitung, Zürich, vol. li, p. 267. 8 Figs.)

The wharfage accommodation on the Lake of Constance was until recently very primitive. The Swiss Railways Association have now constructed at Romanshorn a covered wharf which will act as a dry dock for their steam-boats plying on the lake. One of the chief features is the powerful crane which is described in detail in the earlier part of the article, but the most important part is the ship-lift, which is described and illustrated in the latter part. The apparatus was built in Bern, while the steel structure was made by A. Buss & Co., of Basel, and the electrical apparatus by Brown, Boveri & Co. The lift consists of a wheeled car which runs on an inclined railway, and this begins above the water-line and finishes at such a level below that line as to allow of a vessel being brought over the car, and, by means of the winding-gear, both car and vessel are drawn up into the repairing-shed.

The maximum load which can be raised is 550 tons, the incline of the railway is 6 per cent., and the length of the track about 163·5 yards. Detailed drawings of the car are reproduced, from which it appears that it is built in two parts; the upper end only is used for vessels weighing not more than 300 tons, while both parts are employed together if the weight exceeds that amount. The winding gear has two drums and each has two ropes, and can be worked at a slow or fast speed by means of an electric motor of 25 HP. running at 1,000 revolutions per minute, when supplied with three-phase current at 240 volts pressure and 50 periods.

A test was made with a vessel weighing 550 tons, which was raised out of the water in 1½ hour, and the maximum power developed by the motor was 20 HP. The Author states that the installation has proved very satisfactory.

E. R. D.

Machine-Tools.

(American Machinist, New York, 1907.)

Double-spindle Boring-Machine (p. 720). This boring-machine was specially designed for boring the cylinders of gas- and petrol-engines. The spindle-heads can be adjusted 10¾ to 24 inches

between centres. The work-table is 48 inches wide by 36 inches long, and has both hand- and power-feed with four changes. The cross adjusting screws of the table and of the spindle-heads are fitted with micrometer measuring-gauges. Two cylinders, 8 inches in diameter and 18 inches long, can be bored in 1 hour and 40 minutes.

Motor-driven Boiler-Shell Drill (p. 752). This machine was specially designed for utilizing high-speed steel. There are two vertical standards carrying sliding brackets on to which are mounted two 5-inch circular steel bars which carry two independent motor-driven drill-heads. The bars have a vertical range of 6 feet and are raised and lowered by means of an electric motor. A noteworthy feature is the central position of the drill-head both as regards the carrying-bars and the bearings on these bars; any twisting and side strains are thus prevented and the drill is not thrown out of alignment. Each drill has a vertical adjustment of 6 inches. The machine is 17 feet 8 inches long, 11 feet 6 inches high, and weighs 12,000 lbs.

Portable Crank-Pin Turning-Machine (p. 754). This machine is intended for truing the crank-pins of outside-cylinder locomotives. Advantage is taken of the fact that the outside end of the crank-pin, from which projects the gudgeon-pin, never becomes altered by wear, and can therefore be used to attach a cylinder to act as a guide around which a rotating sleeve can be placed carrying the cutting-tools and provided with a suitable driving- and feeding-mechanism. The machine consists of four parts, each of which can be easily handled even by a boy; it is driven by an air-motor using air at 70 lbs. per square inch pressure, and by its means the largest locomotive crank-pin can be trued in 3 hours without removing the wheels from the engine.

H. R. S.

Machine-Tools.

(American Machinist, New York, 1907.)

Turret-Lathe (p. 901). An unusually full description is given of a new turret-lathe built by Messrs. Pratt and Whitney, capable of doing rod work and machining castings and forgings: its capacity is up to 2½-inch rod by 26 inches, and castings and forgings of 14 inches diameter. The tool-holders have rectangular bodies fitting into pockets in the turret and held by triangular clamps with one stud. There is a large assortment of fittings carried in these tool-holders to be able to deal with special and ordinary tools of all kinds, and they are fully illustrated in the article by means of photo-reproductions. A special chuck is provided for accurate centering operations. It is split in four places and has four jaws closed by a bevel-nut. The jaws are made of mild steel, and to prepare the chuck for a piece of work these jaws are set at a some-

what smaller diameter than the work to be held. A plug is placed in the centre of the chuck and is gripped; the jaws are then machined to the exact diameter required, after which the plug is removed and the chuck is thus set for gripping the work and the piece will run true. The lathe is also provided with a "floating" reamer-holder; the collar in which the shank of the reamer is secured is a free fit in the holder but is held by two spring plugs, and this allows the reamer sufficient freedom to adjust itself to the hole being reamed.

Planer-protecting device (p. 920). A device adopted by the Bullard Machine Tool Company for protecting planer-ways from grit is described and illustrated. Webbing of sufficient width to cover the Vs is securely fastened to the end of the table, the other end of the webbing is wound around spring-actuated spools fixed to a shaft at the end of the bed; the webbing is thus always kept taut. The webbing is kept clear of the ends of the Vs by small rollers placed on balanced levers, which are pushed out of the way by a strip fixed to the webbing close to the end of the table.

H. R. S.

Concrete Machine-Shops at Beverly, Mass., U.S. L. P. ALFORD.

(American Machinist, New York, 1907, pp. 723–9.)

These shops, claimed as the largest in the world, belong to the United Shoe Machinery Company; the major portion was built in 1903–4, and in 1906 a 60-per-cent. addition was made. They consist of three blocks 60 feet wide and 820 feet long; there are four stories in each block, and the clear height under the girders is 11 feet. There is also a foundry 109 feet wide and 390 feet long, and a power-station. These buildings are constructed entirely in reinforced concrete. The outer walls consist of narrow reinforced-concrete columns joined together at each floor and at the coping by horizontal reinforced-concrete stringers. The large spaces thus left are glazed, so that the lighting is very good. There are two rows of octagonal reinforced-concrete columns supporting the main girders, which are also of reinforced concrete. At the first floor the columns are 22 inches across the flats, and this dimension gradually diminishes until it is 14 inches at the top floor. The same core-boxes were used for every floor, any difference in strength needed being dealt with by the amount of reinforcement. The design of the concrete details was carried out under the "Ransome" system and a typical cross-section of one of the main buildings is given, with full dimensions for columns, floors, beams, etc., and the amount of reinforcement in each case. The method of construction employed is that known as the "monolithic," that is, the forms, or core-boxes, for each floor, including its columns, were set up and the concrete was poured at the same time. Each of the main buildings is divided structurally into three independent sections to

allow for temperature expansion, which has been found to vary the space between the sections by ½ inch. A small-scale general plan is given, together with photo-reproductions of the work at various stages and a view of the completed works. These buildings have given satisfaction, no cracks of any importance have appeared; one beam, however, was broken by an accident; a fly-wheel weighing about ¾ ton fell from a height of 4 feet, and shattered the concrete of the beam on which it fell, but the steelwork was uninjured and a repair was easily effected by replacing the broken concrete. The floors are finished with a very rich cement mixture laid on about ¾ inch thick and trowelled to a glass-like surface. This surface is apparently uninjured by oil; at any rate, careful investigation extending over 2 years has failed to show any penetration of oil in the neighbourhood of the machine-tools. H. R. S.

Friction of Lard- and Machine-Oil. WILLIAM K. JERVIS.

(American Machinist, New York, 1907, p. 655.)

This short article gives a description of some experiments made to determine the coefficient friction of lard- and machine-oils at temperatures of 100° F. to 270° F. The testing-machine was in the form of a small drilling-machine and its spindle ran between two opposed brasses placed in a case in a bath of the oil to be tested. The load on the bearings was applied, and measured, by means of a heavy helical spring passing through the side of the case and adjusted by a screw compressing the spring. The rotation of the case was opposed by a tempered-steel spring, so that the amount of rotation of the case was a measure of the friction between the spindle and the bearing, and the rotation was measured on a circular arc fixed to the frame of the machine. The results are given in a tabular form and are also plotted; they are somewhat remarkable for the regularity with which the points fall on smooth curves. At 100° F. the coefficient of friction of both oils is nearly the same, namely, 0·00429 for machine-oil and 0·00432 for lard-oil. As the temperature increases, the friction diminishes rapidly up to about 172° F., at which point, for machine-oil, the diminution becomes perceptibly less and at 190° F. it reaches a minimum, namely, 0·00186. The friction of this oil then increases, slowly at first and then rapidly, and at 270° F. it is 0·00333. This would show that above 190° F. machine-oil disintegrates and the film between the bearing-surfaces begins to break down. In the case of lard-oil the friction diminishes continuously, but somewhat slowly after 180° F. Up to nearly this point the coefficient of friction of lard-oil is about 3 per cent. greater than that of machine-oil; at 185° F. the curves cross and that of lard-oil tends to become asymptotic to the temperature-axis. At 270° F. the coefficient of friction of lard-oil is 0·00164. H. R. S.

Morse Chain on Cone-Sprockets.

(American Machinist, New York, 1907, pp. 73–4.)

This device consists of a series of stepped sprocket-wheels on which a Morse chain works in exactly the same way as a belt works on coned pulleys. The same control of speed is obtained as with a belt with the advantage of positive drive and smaller space occupied for a given power transmitted. To enable the chain to be shifted from one sprocket-wheel to another, three continuous teeth, placed at 120°, are provided. By a slight pressure on the advancing side of the chain it is run down-hill on one of these teeth, and then, by a corresponding pressure on the advancing side at the other cone, it is thrown into engagement with one of the continuous teeth on that cone and finds its seat on the larger wheel. It will be seen, since the teeth in all the wheels must come into line at each continuous tooth, that the diameter of the wheels must be such that the number of teeth is always a multiple of three. The continuous teeth are formed by three continuous properly-shaped steel blades let into the steel blank before the teeth are cut. A figure is given showing how the change from one wheel to another is made by means of the continuous teeth, and there are also photo-reproductions of the application of these pulleys to a horizontal boring-machine and to a milling-machine.

H. R. S.

Standard for Short Involute Gear-Teeth. CHARLES H. LOGUE.

(American Machinist, New York, 1907, pp. 804–5.)

Modern demands, especially for automobile work, require gears of high efficiency and strength, and it has been found that an angle of obliquity of 20° gives a better shape of tooth than the usual 14½°. The tooth thus produced is known as "the stubbed 20° involute" and gives excellent results. The addendum is made to correspond with the addendum of a pitch one or more sizes finer than the actual pitch. The following particulars are given, where P is the pitch :—

Addendum	$= 0 \cdot 7854 \times P$	instead of	$1 \times P$
Dedendum	$= 0 \cdot 9424 \times P$	,, ,,	$1 \cdot 57 \times P$
Working depth	$= 1 \cdot 5708 \times P$	,, ,,	$2 \times P$
Whole depth	$= 1 \cdot 7278 \times P$	,, ,,	$2 \cdot 157 \times P$
Clearance	$= 0 \cdot 157 \times P$	as now used.	

Apart from increased efficiency, it is stated that there is an increase of 40 per cent. in the strength of the teeth, and there is also a saving of material.

H. R. S.

Chemical Engines for Mine-Fires.

(Engineering and Mining Journal, New York, 15 June, 1907, p. 1153.)

Many of the Pennsylvanian coal companies have recently been testing the practicability and efficiency of a chemical fire-engine, devised by A. G. Morse, of Scranton, specially for fighting mine-fires. As the results are highly satisfactory, this means of dealing with a fire is likely to find favour among mining engineers. Though it is not always possible to subdue a fire in this way, it is well known that it can be held in check. The system possesses an advantage over the use of water in that the latter gives rise to volumes of steam which hinder the work of the firemen. The engine consists of two large cylindrical tanks of chemicals mounted horizontally on a platform-truck to run on the mine-tramways. The cylinders are connected, and work through a one-service pipe, to which a hose of any required length can be attached. They are so arranged that one can be refilled while the other is working, thereby giving a continuous service. Convenient lengths of hose are carried on the truck, as well as a supply of chemicals. A photographic illustration is given.

G. G. A.

Alternating-Currents in Coal-Mines. George R. Wood.

(Engineering and Mining Journal, New York, 6 July, 1907, p. 1.)

The alternating-current system is steadily gaining in favour in the United States for work connected with coal-mining. It offers many advantages over the direct-current system, particularly where there are a number of centres of work under one management. Both the first cost and the working-cost are in such circumstances in favour of the alternating system. The cost of installation is lower on account of the higher load-factor, less idle machinery, and lower kilowatt capacity installed to do the same work. The cost of the buildings and foundations per kilowatt is also greatly reduced. Both steam and electric units in the central plant are larger, and compound engines using 150 lbs. per square inch pressure may, on account of the higher load-factor, be employed with advantage. By using water-tube boilers and compound engines, the coal-consumption may be brought down to 3 lbs., or less, per 1 HP.: and it is safe to reckon on one boiler horse-power per kilowatt of electrical capacity. The saving to be effected in fuel, supplies, labour and repairs reduces the cost of power at the switchboard, on an average, from about 2½*d.* per kilowatt-hour for small direct-current plants to ½*d.* or less in the larger central stations.

It is always desirable in underground work to use a comparatively low voltage, not exceeding 300 volts on trolley- and machine-lines. With long transmission-lines, this potential would require, with the

direct system, a large outlay in copper. The alternating system, however, makes it possible, through conveniently placed sub-stations, to work at a low potential without this excessive cost. Where the underground workings are extensive, the sub-stations with step-down transformers and rotary converters may be in the mine, or, if surface conditions permit, directly overhead. The ordinary central power-plant for mining work consists of directly-driven units of such capacity that the requirements of the field will not call for more than five or six units. As a typical alternating-current plant, the Author describes a recent installation at Gary, West Virginia, for the United States Coal and Coke Company. The main power-station is centrally placed, the maximum distance of transmission being about 4 miles. There are here two 740-kilowatt, and two 400-kilowatt, 6,600-volt, three-phase, 25-cycle generators, driven by cross-compound, non-condensing engines. The exciter equipment includes one 25-kilowatt, 125-volt generator, driven directly by a 40-HP. vertical marine-type engine, and there are in addition one 25-kilowatt and one 50-kilowatt motor-generator set. The motors on these sets are wound for 440 volts, and are supplied through oil-cooled transformers stepping down from 6,600 to 440. There are eight sub-stations, one in each mine, in two of which there are two rotary converters, the rest having one each. The converters are of 150-kilowatt capacity each, running at 740 revolutions a minute, and delivering direct current at 725 volts. Each rotary equipment includes three 55-kilowatt, 6,600- to 172-volt transformers, all of the oil-filled, self-cooling type requiring practically no attention.

G. G. A.

Park Automatic Loader.

(Engineering and Mining Journal, New York, 22 June, 1907, p. 1189.)

The latest advance in the application of power in substitution for hand-labour is the Park machine "mucker," or automatic loader, manufactured by the Railway Materials Company of Chicago. Its use is to pick up and discharge into trucks broken stone, coal, coke, gravel and other loose materials on the surface, and the broken rock from a blast at the breast underground and in all excavating work except sinking. This it is doing successfully in the underground workings of the Gold Lions Mines Company at Red Mountain, Colorado, in a fraction of the time required to clean by hand after a blast. In essential features the machine consists of a heavy steel truck driven by an electric motor, a steel shoe-plate, travelling scraper-arms for gathering material from the ground, and a short and wide conveyor-belt for transferring the stone to the wagons. The power for driving the machine may be taken from any convenient circuit. The loader is brought to the working-face by its own power, and is steered and managed like a steam-roller. It

is capable of doing the work of a steam-shovel in the narrow workings of a tunnel. The machine is built in two sizes, the larger being 6 feet 6 inches high and 5 feet 6 inches wide, and the smaller 5 feet 6 inches high and 4 feet wide. The working capacity of the larger machine is 50 to 120 cubic feet per minute, according to the character of the materials to be dealt with; that of the smaller machine being 30 to 75 cubic feet. They are necessarily of massive construction, the larger size weighing about 7 tons and the smaller about 5 tons. The shovels, which project 18 inches and 12 inches respectively for the two sizes, are worked by a 10-HP. motor, which also propels the machine, and a 3-HP. motor for driving the conveyor-belt. The shovel-apparatus consists of a heavy interlocking endless chain revolving about a sprocket-wheel at the head and a traction-wheel at the foot, carrying shovels capable of lifting 16-inch cubes, at every 4 inches. Photographic illustrations are given of the machine.

G. G. A.

Self-Discharging 50-*Ton Mineral Truck.*

(Annalen für Gewerbe und Bauwesen, Berlin, 15 August, 1907, pp. 75–6.)

A hopper-truck, carried on fore-and-aft bogies, with a capacity of 50,000 kilograms, manufactured by the Koppel Company for the Haspe Iron and Steel Works in Westphalia, is described and illustrated. This truck, intended for the transport of slag-sand and debris from the works, is constructed wholly of steel, and is remarkable for the absence of underframe. The hopper, of which the sides are formed as trusses, is supported direct on the bogies, which are of the "diamond" carriage type. The emptying takes place at the bottom of the hopper by means of double sliding doors, placed horizontally to command an opening of 6 feet 6½ inches by 2 feet 7½ inches wide. This arrangement has the advantage of making good use of the space, and permits of a favourable position being chosen for the centre of gravity; moreover, by allowing for the regulation of the size of opening, it becomes possible to modify the rate of discharge. The sliding doors are driven by chain-gear and can be opened and closed from either side of the truck. The extreme length at top of hopper is 28·87 feet, width 7·67 feet, height above rail-level 11·81 feet, weight empty 16·85 metric tons, weight of load 50 metric tons. It is stated that these trucks are easy in draught, and take sharp curves and gradients of 1 in 60. They can be emptied in 2 to 3 minutes at a cost of ¼*d.* for labour.

G. R. R.

Concrete Shaft-sinking through Quicksand.

(Engineering and Mining Journal, New York, 29 June, 1907, p. 1239.)

A cylindrical concrete shaft has just been sunk by the Foundation Company of New York at Biwabik, Minn., through quicksand without any serious delay in the operations. The depth of the sinking through this stratum is about 126 feet. This shaft is unique in character, and is remarkable for the novelty of the method of sinking adopted. Briefly it consists in placing in position a steel shoe to which are attached an inside and an outside form between which concrete is run. The diameter of the inner form is in this instance 14 feet 6 inches, that of the outer being 22 feet 6 inches. This allows the formation of a hollow cylinder of concrete 4 feet thick to form the walls of the shaft. Inside the two forms there is a hollow steel cylinder 6 feet in diameter in 8-foot sections, flanged top and bottom. Two successive sections were joined by bolting through the flanges. Through this cylinder all the sand was brought up by dredges. These are either of the clam-shell or the orange-peel type. No attempt was made to remove the water. When the clam-shell bucket had removed sufficient sand the shoe was allowed to drop 8 feet, and that height of concrete was then poured in between the forms. Reinforcing-rods, of special high-carbon steel 1 inch square in section, were first placed all round and near to the outer form, every fourth rod being "staggered." These rods were bent to a right angle 6 inches from the ends to enable them to hook on to the next set. A further reinforcement of a steel band was placed at every 4 feet. The dredging was then continued until the excavation was sufficient for another 8-foot drop, when the same operations were repeated. After each drop, of course, new sections of the forms and the steel cylinder were added. Means were provided for removing the water by compressed-air in the event of boulders being encountered, and also for anchoring the shaft when the firm rock was reached.

G. G. A.

Peat-Coke. MAX TOLTZ.

(Journal of the Association of Engineering Societies, Philadelphia, vol. xxxviii, pp. 233–43.)

According to state geologists the main peat-beds or bogs in the United States are to be found in the middle west, and in fifteen States reported the total area amounts to 7,000,000 acres with an average depth of 10 feet. A rough estimate for the whole of the United States would be about 10,000,000 acres. Each acre 1 foot deep will yield 402 tons of air-dried peat containing 20 per cent. of water, and this when converted will yield 164 tons of the best coke, having the same heating value as charcoal. At the present rate of

coal-consumption the author estimates that the peat, if turned into fuel, would last 60 years, peat-coke having 26 per cent. more calorific value than coal. The peat is excavated by machines which deliver it in briquettes 4 inches square and 10 to 12 inches long, which are then air-dried. The peat as it comes from the bed contains 80 to 90 per cent. of water, and this is reduced to 40 or 50 per cent. by the drying process. It is then placed in air-drying chambers, where, by the heat given off by the coke-converting ovens, the moisture is further reduced to 20 per cent. To obtain 100 tons of peat it is necessary to dry out 160 tons. The coke-oven gases are collected and cooled to 160° in a mixing-chamber, as they are too hot if used direct. After the drying, the cooking process is carried on in ovens with elliptically-shaped retorts of firebrick in the lower half, and of cast iron with a thin outside lining of firebrick in the upper half. In one of these ovens 18 tons of air-dried peat can be converted into coke, and the highest temperature in the oven is 1,100° F. The coke is hourly drawn off into air-tight steel wagons where it is allowed to cool. The peat-refuse and such as is not suitable for cooking can be used in a gas-generator.

One hundred tons of dried peat will yield 4·5 tons of tar and 46·6 tons of impregnated water, out of which ammonia, wood-alcohol, paraffin, and other substances can be obtained. This process of converting raw peat into coke was invented by Martin Zieghn. The most recent plant of the kind is at Beuerberg, Bavaria, with a capacity of 200 tons of coke.

R. S. B.

Smoke-Disposal from Smelting-Works. HERBERT LANG.

(Engineering and Mining Journal, New York, 29 June, 1907, p. 1226.)

The question of getting rid of the smoke from smelting-works is beginning to attract attention in consequence of the growing hostility of the public in many districts. The Author was brought to face the difficulty in connection with copper-smelting works situated on the edge of a highly cultivated tract of country. A hill, 1,000 feet high, rises at a distance of 3 miles from the site, and it was proposed to convey the smoke through ducts from the furnaces to the top of the hill where it might be discharged into the atmosphere without risk of damage to the vegetation below. The Author believes that all similar problems can be dealt with in this way whenever there is a relatively barren hill within 3 or 4 miles of the works. The question turns on the cost of the duct and the power required to maintain the flow of the noxious gases within it. Though at first sight the material seems unsuitable, the duct may be constructed of wood, and, for cheapness, of A, or inverted V, section resting on the ground, the earth forming the bottom of the triangular duct. This, in form like the steep roof of a house, would

be easy to construct, requiring little carpenter's work, and would shed the rain and resist the wind very well. The outside would be battened. The main objections to the use of wood are: its liability to warp under the heat of the sun, and the inefficiency of the triangular section as compared with the circular. Before entering this duct the hot gases will have cooled down to about 125° F., by being passed through a length of sheet-steel dust-catchers and flues. The cost of such a duct, having a cross section of 24 square feet, is estimated to amount to £2,200 per mile in that locality, where an iron tube of No. 8 sheets, 5 feet in diameter, would cost £3,000. In any case forced draught would be necessary. The Author's plan is to introduce suction-fans, of the Sturtevant "Monogram" type, in the line at intervals of about a mile, to be driven by electricity from a parallel line of wire. Doors would be provided to give access to the inside of the duct for cleaning purposes. The tube should be duplicated at the lower end where the fumes tend to precipitate most copiously, so as to allow one side to be cleaned while the smoke is passing through the other.

G. G. A.

Cutting Metals by means of Oxygen. LÉON GUILLET.

(Le Génie Civil, Paris, vol. li, pp. 241–4.)

When a jet of oxygen is allowed to impinge upon iron previously raised to a high temperature it combines with the iron, an oxide of the metal being formed, which, being more readily fusible than the iron itself, passes away as it is formed, thus exposing fresh surface to the action of the jet. In the earlier experiments with this process, the metal was first heated by the oxy-hydrogen jet; on the required temperature being obtained the heating was stopped and the jet of oxygen brought into play, but more recently the heating has been carried on simultaneously with the oxidizing, the result being a much cleaner cut. In one of the latest forms of the apparatus the heating is effected by an oxy-acetylene jet issuing from an annular space, in the centre of which is the jet of oxygen. The extreme rapidity with which iron or steel can be cut by this method is illustrated by the following facts:—(1) An armour-plate 6·29 inches in thickness was cut in two for a length of 3·28 feet in 10 minutes; (2) the cutting of manholes in plates 0·78 to 1·17 inch in thickness occupied 4 to 5 minutes each; (3) in 7 minutes a groove 3·28 feet in length by 1·56 to 2·34 inches deep was cut in a slab 11·7 inches in thickness, while in the same piece of metal, by the use of a pneumatic chisel, the cutting of a groove of only one quarter the depth and 3·77 feet in length occupied no less than an hour. The process has also been found very efficient in removing the heads of rivets when a seam is to be undone, the head of a $\frac{7}{8}$-inch rivet being burnt off in 12 seconds.

It might be thought that the metal contiguous to the cut would be seriously depreciated in value, but a series of carefully-conducted experiments conclusively show that the depreciation is exceedingly small.

I. C. B.

Expansion of Metals by Heating and the Formation of Cracks.

CARL SULZER.

(Schweizerische Bauzeitung, Zürich, vol. l, p. 41.)

Much discussion has arisen respecting the cause of the production of cracks in metals by heating, and this is especially the case in boiler-work. The case cited in this article is that of a Cornish boiler 30·6 feet long and 6·56 feet in diameter for a working steam-pressure of 105 lbs. per square inch, and a test-pressure of 180 lbs. per square inch. It was built by Messrs. Sulzer Brothers, of Winterthur, in 1899, and the material used was Siemens-Martin mild steel. Tests of the plates at the rolling-mills showed an average breaking-load of 23·45 tons per square inch, and an average elongation of 30·1 per cent. The chemical analysis showed the carbon to be 0·048 per cent. and the boiler was built in the best manner with all rivet-holes drilled in position after the plates were bent, and the rivets were closed by hydraulic pressure.

The boiler was put to work in January, 1900, and was used regularly from that date. A number of leakages showed themselves in 1905, and far worse ones in 1906 and in February 1907, and all these were stopped by caulking. In April, 1907, they became so bad that caulking was useless. Careful inspection showed cracks in the circumferential seams of the shell-plates which necessitated a new shell. It appeared that the boiler had been very much forced, as much as 8·2 lbs. of water being evaporated per square foot of heating surface per hour, which is nearly double the rate allowed by the makers. Cleaning was done very rapidly and the boiler set to work again at once, so that the changes in temperature were very great, but it did not appear that excessive scale had been allowed to form. Illustrations show what serious cracks had been formed between adjacent rivet-holes. Test-pieces cut from the defective parts showed that the material was still as good as when first used, and the Author concludes that the damage was not caused by the internal steam-pressure, but solely by the excessive heating of the external shell-plates through undue forcing and very heavy firing.

E. R. D.

Testing Metals: New Mechanisms and New Methods.

P. Breuil.

(Revue de Mécanique, Paris, 1907, pp. 313–63.)

In 1899 Mr. G. Charpy published in the *Revue de Mécanique* a study he had made of the above subject. Since that time special attention has been given to the matter by numerous investigators and inventors and several new methods of testing metals have been proposed, with the object of exhibiting certain good and bad qualities which it is said the older methods are unable to detect. The Author sounds a note of warning, and expresses the opinion that in many cases there has been too much haste in accepting the pretensions of the new methods. In particular he refers to impact-testing, and points out that, because this method detects brittleness, the conclusion was rapidly come to, in certain quarters, that it could replace the tensile tests, and that in fact the tensile method was useless for selecting materials and ought to be abandoned. He expresses approval that a proposition to this effect made at the Congress of the International Association for Testing Materials held at Brussels in September, 1906, was rejected; he proposes to revert to the matter in a future article. Methods of testing are divided into two groups, namely, those in which the test-piece is slowly stressed and those in which it is rapidly stressed; the former are called "static" and the latter "dynamic." The machines for carrying out the tests are grouped into those capable of testing a finished article, such as a chain- or a bridge-link, and those able to deal only with comparatively small test-pieces; the latter group are intended for determining the quality of the metal before it is worked up into a finished article. In this number of the *Revue de Mécanique* only tensile-testing machines for carrying out static tests are considered. Several very large machines with capacities of 250 to 400 tons are described, such, for instance, as a horizontal 300-ton machine of the Conservatoire des Arts et Métiers, built by Messrs. Buckton, of Leeds. The sensibility of this machine is such that the equilibrium of the beam is disturbed by a change of 4 lbs. when the load is 100 tons; that is, the sensibility is 0·02 per cent. A 250-ton machine belonging to the Association des Industriels de Belgique is described, as also a horizontal Riehlé machine of 400 tons, as well as another vertical 250-ton machine by the same makers, designed by Professor Talbot and erected at the University of Illinois. The Author criticizes this vertical machine and thinks that the placing of the test-pieces in position must be difficult; he refers, however, to special arrangements by means of which the sensibility has been rendered very great. The Author suggests that these large testing-machines are not utilized as much as they might be to investigate matters of great importance, as for instance, in studying the elasticity, deformation, strength, etc., of large chains and steel-wire ropes. He points out that some experiments on small wire-ropes

published in 1906 in *Dingler's Polytechnisches Journal* show that they do not follow Hooke's law, and that the length of time of application of the load had an important effect on the elongation. He asks what the law for large wire-ropes is and answers: "No one knows, nor tries to find out, although it is a matter of the highest importance, seeing that large wire-ropes do sometimes break in service." A large number of smaller testing-machines of 1 to 50 tons capacity and of various designs are described and illustrated, and the article concludes with the description of several forms of automatic recorders. A measuring-device called a "Boîte de Mésure" is also described at some length, the principle of which is that the pressure produced in a small vessel by a piston carrying the load is measured by means of a pressure-gauge; the difficulty lies in reducing the friction of the piston to a minimum. In one form, designed by Martens, the liquid is contained between two thin soft brass disks, brazed at the edges. The piston is supported by these disks and the load is applied to it by a knife-edge. The pressure-gauge is read by means of a reflecting-mirror, and in this way great sensibility is obtained. This measuring-device has been utilized by Martens for a variety of purposes such as the measurement of wind-pressure and in studying the propagation of pressure in narrow tubes. In another design the pressure-gauge readings are obtained by means of a delicate multiplying-lever.

H. R. S.

Testing the Hardness of Metals and Materials.

W. I. BALLENTINE.

(American Machinist, New York, 1907, pp. 698–9.)

The Author points out that the use of high-speed steel has required a greater knowledge of the physical qualities of the materials being cut than was deemed necessary in the case of the older tool-steels, and he believes that the greater output obtained with the new steels is in considerable measure due to this better information, and had it been applied to the old carbon tool-steels astonishing results would have been obtained. The hardness of the material being cut is one of if not the most important elements determining the cutting speed, but the Author says that the three methods now in vogue to determine this factor are more suitable for a laboratory than for a workshop, because the material to be tested, or a sample of it, has to be brought to the testing-apparatus. What is wanted is an easily portable instrument which can be applied to the material as it lies in the shop. The Author has devised such an instrument consisting of a steel tube of $\frac{7}{8}$-inch diameter in which is placed a cylindrical drop-hammer. This hammer can be held up at the top of the tube by a spring catch, and at its lower end it carries a small anvil to which is fixed by means of a screw-cap a soft

metal recording-disk. At the lower end of the tube, and sliding freely, is a "test-pin" having a hemispherical point for penetrating the metal to be tested. The principle of the instrument is that the energy due to the fall of the hammer is absorbed partly by the penetration into the material being tested and partly by compressing the soft metal disk; the greater the hardness of the material the greater will be the compression of the soft metal disk. These disks are made to a standard thickness; it is only necessary, therefore, to measure the thickness after compression, and the Author takes the compressed thickness expressed in $\frac{1}{1000}$ inch as an index to the hardness. Thus a material giving a figure 148 would be harder than one giving 156. An example of figures obtained on a cast-iron wedge is given.

H. R. S.

Cleaning Iron and Steel by Chemical Means. E. LEMAIRE.

(Le Génie Civil, Paris, vol. 1, pp. 448–50.)

The composition of acid baths for cleaning iron or steel varies much, as, not only do the results to be obtained differ in accordance with the treatment which the cleaned surface is subsequently to undergo, but even for the same after-treatment different factories adopt different mixtures, the composition of each being usually kept secret with the utmost care. The crust or scales formed on the surface of iron or steel consist of the various oxides of iron (FeO, Fe_2O_3 and Fe_3O_4), the latter differing from the others in that it is formed during the process of manufacture, and appears to do little harm provided that it adheres so closely as to prevent moisture entering between it and the main body of the metal—a state of affairs which is of frequent occurrence. The bath for chemically cleaning the metals in question should be capable of dissolving at any rate FeO and Fe_2O_3 without attacking the metals themselves, but as these oxides are electro-negative with respect to the main body of metal this is almost impossible of attainment, and all that can be hoped for is to secure a composition which acts on the metal to the minimum extent, and above all acts uniformly, i.e., does not produce pitting. A curious fact has been noticed in connection with cleaning iron or steel by acids, viz., that the physical properties of the metals are altered in a marked degree, the material becoming much more brittle and losing its elastic properties. This action is especially marked when hydrochloric acid is used, while with nitric acid the effect is by no means so apparent, and with sulphuric acid it occupies an intermediate position. It is suggested that this alteration in physical properties is due to the nascent hydrogen combining with the iron, this being borne out by the diminished effect of the nitric acid which in itself is an oxidizing agent, and also by the same diminished effect produced by certain other oxidizing agents introduced into the hydrochloric or sulphuric-acid

baths. The article concludes by pointing out the beneficial effect of adding to the acid bath arsenious oxide (As_2O_3), which not only almost entirely prevents the deterioration in physical properties, but also notably diminishes the pitting, leaving the surface in admirable condition for subsequent treatment.

I. C. B

New Views on the Corrosion of Iron.

(Engineering Record, New York, 6 July, 1907, pp. 1-2.)

The text-books affirm that carbonic acid is responsible for rust. Iron is attacked by it with the formation of carbonate, which is then acted on by water and the oxygen of the air, to form the red hydroxide known as rust, the carbonic acid being then set free to take up its destructive work. According to Dr. Cushman, who, in connection with the United States Department of Agriculture, has been investigating for years the causes of corrosion in fence-wire, the above theory must be abandoned. The most startling results of these inquiries are that oxygen plays only a secondary rôle, and that the best preventatives of rust are to be found among the most effective oxidizing agents known, such as chromic acid and its salts. That these act as inhibitors has been known for some time, but no explanation of the curious phenomenon has been hitherto offered. The high authority of Dr. C. B. Dudley is quoted as stating the discovery to be the most important contribution to the metallurgical problem for the last quarter of a century. The article is an instructive comment on Dr. Cushman's electro-chemical theory, which is that the first attack on iron is not made by oxygen, even in the presence of water, but by hydrogen in the form of the hydrogen ion. The importance of the subject of preventatives, from this point of view, is insisted on, more especially in view of the rapid corrosion of boiler-tubes used in connection with turbine-engines, owing to electrolytic action. The subject is resumed in the *Engineering Record* of the 10th August, 1907.

C. O. B.

Difficult Steel-Casting and the Cores used for the Mould.

JOHN GREGSON, Jun.

(American Machinist, New York, 1907, pp. 429 and 666–7.)

These articles describe a steel-casting, weighing 10,540 lbs. and forming the cutter for a suction-dredge for the United States Government. It is believed to be one of the most intricate large castings ever made in steel. It has somewhat the appearance

of an octopus with eight legs or blades; it is 6 feet high and has a diameter of 7 feet 6 inches. Each blade is wedge-shaped and runs in a spiral from the nave to the base. At the base each blade ends in a small plate, and there the blade is 2½ inches thick, tapering to a knife-edge in 13 inches; near the nave each blade is 4½ inches thick, tapering in 18½ inches to a knife-edge. No pattern was made, but the mould was built up by means of nineteen cores made from five different boxes. The first core was rammed up on the "drag," or base, and was a simple truncated cone; it carried another core forming the under portion of the nave, and had a "print" to take the core forming the hole through the nave. Around these two cores were placed eight cores containing the lower portion of each blade, and on the top of these were placed eight more cores, breaking joint with them, and each having on one side the front half of the remainder of a blade, and on the other side the remainder of the back half of the next blade. The latter cores also form the top portion of the nave, and they were somewhat difficult to place, as they overhung in two directions; the last one, which formed the key, had to be very carefully fitted in. The work was carried out with great accuracy and it took some 4 weeks to assemble the parts. The nineteen cores were rammed up in a flask 12 feet square by 11 feet high, and a sound casting was obtained at the first trial. A dimensioned section of the mould is given, and a photo-reproduction of the finished casting.

H. R. S.

Tantalum Steels. L. Guillet.

(Comptes Rendus de l'Académie des Sciences, Paris, 1907, vol. cxlv, pp. 327–29.)

In view of the fact that special qualities have been claimed for tantalum steels, and that many patent specifications seem to show that the addition of tantalum considerably improves the properties of steels, the Author has made an investigation to determine the point. He examined the properties of four specimens, in which the respective percentages of carbon, manganese and silicon remained almost constant, while that of tantalum varied between 0·9 and 1·05. The results of the investigation show that tantalum steels, at least when they have a low percentage of carbon, do not exhibit any property which merits special attention.

W. C. H.

Radiation from and the Melting-Points of Palladium and Platinum. C. W. WAIDNER and G. K. BURGESS.

(Bulletin of the Bureau of Standards, Washington, 1907, pp. 163–208.)

All measurements of temperature up to 1,200° C. are now referred to the gas-thermometer and with nitrogen-gas the limit of accuracy at this temperature is 5°; beyond 1,500° C. the scale must rely, for the present, on extrapolation based on the radiation laws. For the practical use of the scale it is desirable to have reproducible fixed points, and to this end the melting-points of palladium and platinum have been determined by many observers during the last 2 years. Unfortunately there is a variation of 70° C. between the best of these determinations, and the true values may still be regarded as uncertain by 40° C. In the investigation recorded in this Paper, three methods have been used, namely: (1) the black-body method in which the palladium or platinum was melted in an electrically-heated iridium-furnace, the temperature being measured by an optical pyrometer; (2) the surface-radiation method, in which the black-body temperature of electrically-heated ribbons of the metals was measured at the instant of melting; and (3) the thermo-electric method based on extrapolation. The former was the method most relied on, and depended on the application of the Wien equation: $J = c_1 \lambda^{-5} e^{\frac{-c_2}{\lambda T}}$. The black-body used was a modification of the form devised by Lummer and Kurlbaum, and an account is given of the calibration of the pyrometer-lamps, of the determination of the absorption-factors and of the optical homogeneity of the glasses used. Check measurements were taken by determining the melting-point of a minute piece of gold placed on the platinum- or palladium-ribbon, and a very close agreement was obtained. The iridium-furnace used for the first method was coated inside and out with a mixture of refractory earths due to Nernst, in order to avoid evaporation of the iridium and the contamination of the palladium and platinum. In the case of palladium the average of a number of observations, reduced by means of the Wien equation, gave 1,540° C. as the melting-point, but by the method based on direct extrapolation, using the calibration equations of the pyrometer-lamps, the figure 1,547° C. was obtained. In the case of platinum the average of groups of observations, some taken with absorbing mirrors, others with a sector disk, and with red, green and blue light, varied between 1,740° and 1,746° C. The surface-radiation method yielded somewhat higher melting-points, namely, for palladium 1,561° C. with red light, and 1,569° C. with green light; in the case of platinum the corresponding figures were 1,748° and 1,747° C. The thermo-electric method gave in general lower results, grouped, in the case of platinum about 1,706° and 1,731° C. depending on the formulas used to express the relation between temperature and electromotive force; the lower figure being obtained

from the type of equation (Avenarius) used by Harker (his figure is 1,710° C.). The article concludes with a comparison of the results obtained by the various methods used, and the final values of 1,546° C. for the melting-point of palladium, and 1,753° C. for that of platinum are given. It is pointed out that the wide fluctuation in these melting-points, as obtained by various observers during the last 2 years, necessitates further experimental work, and the Authors have therefore planned to attack the problem from the basis of the Stefan-Boltzmann law.

H. R. S.

Magdeburg Water-Supply.[1] Dr. Peters.

(Zeitschrift für Hygiene, Leipzig, vol. lvi, pt. 3, pp. 400–34.)

The town of Magdeburg has for hundreds of years derived its supply of drinking-water from the Elbe, and until some 20 years ago no evil results of any consequence have ensued. The water was not always very palatable, but the town has been fairly free from serious epidemics. Owing to the rapid industrial progress of the districts situated high up the river, the increase of population, the manufacture of soda, potash and sugar, and the development of the mines, the Elbe became more and more polluted, and then in 1892 the question of the provision of unobjectionable water suddenly became an urgent one. The difficulty arose from the inflow of salt water into the mines in the vicinity of Eisleben; this was caused by an underground communication between the salt water of the Oberröbling Lake and the mine workings. This water was pumped out and flowed into the River Saale, and was thence discharged into the Elbe, and it was calculated that the inflow carried with it 200,000 to 300,000 cwt. of salt daily, rendering the river-water quite unsuitable for drinking and manufacturing purposes. In the winter of 1892–93, when the water was very low, the salt amounted to 181 parts per 100,000, and the proportion remained almost as much through the whole of the following year. The town was called upon by the Government to provide a better supply, and the Author discusses the various plans put forward from time to time. The whole controversy turns now on the question whether it is possible to render the water of the Elbe suitable for drinking by some special treatment, or whether a wholly fresh supply must be procured. The opinions of the different authorities who have from time to time reported upon this subject are considered, and on the question of the provision of a supply of underground water special reference is made to the Breslau disaster in 1906. The measures taken to search for water in the vicinity of Fläming are described, and the results obtained by means of sixty-nine borings are discussed. These trials were interrupted by an inundation in

[1] See also Minutes of Proceedings Inst. C.E., vol. clvii, p. 436.

July, 1905, and no definite facts have been obtained as to the adequacy of the water. In the meanwhile, wells have been sunk in the town which yield a small supply of wholesome water, and considerable sums have been expended upon the improvements of the existing filter-beds. It appears doubtful whether, in case that a sufficient supply of good water is obtained from a distance, the authorities of Magdeburg could call upon the owners of these wells to cease to use them and take only the water from the town mains. The Author states that his object is to place before those who may be interested a full account of what has been done in the matter of the Magdeburg water.

G. R. R.

Madrid Water-Supply. Official Reports of the Collapse of a Reinforced-Concrete Tank at Madrid in April, 1905.[1]

(Report of the Special Committee appointed by the Spanish Government, 1905.)

The report is in Spanish, and a typewritten copy, consisting of 69 pages numbered 108 to 176, is in the Library of the Institution.

Pages 108 to 126 contain a minority report signed by two members, pages 127 to 134 a majority report, pages 135 to 161 a detailed examination of all the causes which led to the disaster, and pages 162 to 176 an appendix to the last-named portion. The collapse took place on the 8th April, 1905, and the report is dated June 1905.

The project consisted of a large reservoir, constructed entirely of reinforced concrete. The roof was formed of thirty-six parabolic arches, each 288·6 feet long by 19·7 feet span; each arch rested on two continuous beams and each beam was supported by twenty-one columns 26·24 feet high. The design was calculated for vertical and static loads only, and the collapse took place before the construction was quite finished and before any water had been let in.

The majority report states that it was very important to examine the materials in the positions where they fell, but these had been moved to enable the dead and wounded labourers to be got out. It was publicly reported that a subsidence of the sub-soil had taken place. A plan of the collapsed portion was prepared by the engineers of the canal, and photographs were taken. The commission discussed the following matters as possible causes of the disaster:—1. Defective cement; 2. Defects in materials and labour; 3. Defective design of the structure.

With regard to subsidence, careful inquiry was made as to the work done to the original site, that is, to the bottom of the reservoir to make good defects caused by old wells and ancient waterways, as it was believed that the points where the ground had been made up

[1] Minutes of Proceedings Inst. C.E., vol. clxx, p. 466.

coincided with the points where the initial defects had appeared in the structure.

Special examination of the concrete bottom was made with levelling-instruments, and it was found that the bottom had not sunk at all. With respect to the cement, the aggregate and the steel employed, careful tests were made, and these showed that all the materials were up to the standard required by the specification. The committee themselves selected the portions of the pillars and roof from which the test-pieces were cut. Careful examination was also made to see whether the correct proportions of cement and sand had been employed, and it was found that the specification had been followed. The tests showed that the concrete varied considerably in strength.

After completion the roof was to be covered with a layer of earth 9·75 inches deep, and this was calculated to weigh 61·5 lbs. per square foot; allowance had to be made also for snow and rain, and for the weight of workmen doing repairs. The designers took 143·5 lbs. per square foot as the load on which they calculated the structure, and the test-load prescribed was 215·25 lbs. per square foot; but this was only required to be put on over a representative portion of the roof, as it would have entailed too much expense to have covered the whole area with such a test-load.

The loading with the earth covering proceeded from the 5th to the 8th April, when the collapse occurred.

The committee state that, if the possibility of lateral flexure in the columns be taken into account, then the factor of safety for the test-load was 3·4 to 3·8; for the load actually put on it was 4·2 to 4·9; and for the load taken in the original calculations 4·8 to 5·3, and for the permanent loading of 9·75 inches of soil, when dry, it would be 8 to 9.

The conclusion is that the beams, vaults, and pillars had too small a factor of safety, but the committee state that they ought in justice to point out the difficulty of making certain of uniformity of the quality of reinforced concrete, and therefore they think it desirable to fix a higher factor of safety than was here the case. With respect to the actual deposit of earth on the roof, the committee had to rely on the evidence of the persons who were responsible for carrying out this work. The committee were of opinion that the gradual deposition of earth on the part which gave way produced stresses in the structure which were not vertical and were not allowed for in the original calculations. The accumulation of these stresses gave rise to local failure, but the exact location of the first failure cannot be definitely stated, and in the selection of the most likely spot the majority and minority reports differ. It is agreed, however, that as soon as the local failure occurred excessive stresses were produced in neighbouring parts causing a general collapse of a large part of the roof. It is pointed out that methods of calculation for reinforced-concrete structures are not yet definitely standardized.

Certain damage occurred to the structure in June, and this was said to be caused by the high temperature at the end of May,

causing expansion of the structure which had not been protected by the prescribed covering of earth, but the majority consider that so far as the collapse in April of the roof of the fourth compartment is concerned, the variations in temperature had little or nothing to do with the disaster.

The majority state their conclusions as follows:—

1. The disaster began by the breakage of one or several of the elements of construction, pillar, beam, or vault.

2. The probable causes were—

(*a*) The creation of forces which were not vertical by the distribution of earth over the vaulting. The structure was designed to withstand vertical stresses only.

(*b*) In a smaller proportion the quality of the cement-mortar due to variation in mixing may have had some effect.

3. One or other of the elements having given way, the general collapse followed.

The Authors of the minority report point out that the fall of one column would in their opinion only cause damage at most of the four vaults surrounding it, and would not cause extraordinary stresses on more distant vaults. As to the calculations for the girders they differ in opinion from the views of the majority. With the calculated load they find the tensile stress in the steel would be 9·5 to 13·3 tons per square inch, and the main compressive stress would be 327 lbs. per square inch. From these calculations they are of opinion that the structure as designed and constructed was satisfactory for the calculated load and therefore much more so for the permanent load. It is to be observed that the structure would not be subjected to horizontal stresses, and that when the water was in, the changes in temperature of the air would have no effect.

There is no doubt in the minds of the Authors of the minority report that the structure was exposed to horizontal stresses owing to differences of loading, movements of men and carts, and also to changes of temperature which had a prejudicial effect. There was a difference of 72° F. between the minimum 32° F. and the maximum 104° F. in the 24 hours before the collapse, and during this time part of the vaulting was not protected by soil. Deformations in the vaulting had been observed before the disaster, which could only be due to contraction and expansion under varying temperatures. They then give a detailed description of the wreckage, and state that all tends to support their theory of a horizontal thrust which could only be produced by expansion and contraction. They consider that the vaults in the centre first gave way and the damage spread gradually to the eastern and western walls. Their conclusions are:

1. The disaster began by the thrusting upwards of some of the vaulting along a line near the centre, and this dragged two rows of columns together at the top, and the stresses caused the adjoining vaults to fall and eventually let the whole roof down.

2. The initial thrust was due to three causes:—

(*a*) Expansion and contraction caused by the variations in temperature.
(*b*) Differences in the loads of earth on adjoining vaults.
(*c*) Shocks and vibrations caused by the workmen and by carts.

E. R. D.

Buttressed Concrete Dam.

(Engineering Record, New York, 24 August, 1907, pp. 214–5.)

This unusual design of a dam buttressed by triangular counterforts against water-pressure is the result of raising a masonry dam to meet extra requirements, by a concrete addition. A longitudinal groove or dovetail was cut into the top of the original wall as a key to the new work, which is of 1 : 2½ : 5 concrete, and rising 7 feet above the old crest (26 feet high) is carried down the down-stream face, thickening the wall throughout, but retaining the slight batter. Owing to this, and to the difficulties of extra transport of materials which would be incurred if a solid flatter slope were given, the counterfort principle was adopted. Moulds for each counterfort were provided so that continuous building could be carried out, giving greater solidity to the work than if shifting had been resorted to. The original estimate is given, the actual cost having been about 7 per cent. less.

C. O. B.

Cableway used for the Construction of a Dam in Nevada.

(Engineering Record, New York, 3 August, 1907, pp. 133–4.)

The dam is 1,300 feet long and 35 feet high. The site being practically inaccessible the design, originally contemplated for solid masonry or concrete, was changed to that for a loose rock construction puddled on the upper slope, the material being quarried on each side and delivered by a cable crane system of somewhat novel type. The dam alignment is of a flat V-shape with unequal legs 525 feet and 765 feet each, meeting in a small hill on which a telescopic pivoted strut is erected bisecting the angle to take the angular cable-stress. The single Victor cable, 2 inches in diameter, has a capacity for a 5-ton load, and a breaking-strength of 200 tons. It is 1,290 feet long, and is attached at both ends to oscillating travelling-shears, which give it sufficient transverse movement to enable it to command the maximum width of the dam, 75 feet. Each end-tower is inclined away from the centre of the dam so as to exert a constant tension on the cable and has suspended from its apex a 40-ton counterweight capable of sufficient vertical motion to allow the top of the tower to revolve through an arc sufficient to permit the adjustment of the

angle to the position of the load with a constant tension. Both towers are seated on single-line rails perpendicular to the axis of the adjoining cable, and when it is necessary to move either one, the other can be adjusted until the counterweight is seated with its bearing on the ground. The deflections due to dead-weight and maximum load on the long and short spans, are 6·19 feet and 39·54 feet, and 2·41 feet and 23·26 feet respectively, the maximum cable-stress being 42 tons.

The design of the towers, which are 38·9 feet in height and chiefly of timber, is described and illustrated, also details of the gearing. The hoisting-apparatus is rated at 40 HP. and can develop a maximum of 60 HP., the speed for the maximum load being 80 feet per minute for hoisting, and 1,500 feet per minute for traversing. A 50-HP. G.E. dynamo, driven by a 75-HP. engine, furnishes the current. It is estimated that the cost of delivery will be 1*s.* 0½*d.* per cubic yard, with wages at 2*s.* 1*d.* per hour.

C. O. B.

Influence of Free Carbonic Acid in Water-Supplies.

Dr. Hartwig Klut.

(Gesundheits-Ingenieur, Munich, 10 August, 1907, pp. 517–24.)

Increasing attention is being directed to the important part played by dissolved carbonic acid in the corrosive action which takes place in iron vessels or receptacles in which water is stored, and in the influence of this acid on the solubility of lead. The recent researches of Messrs. Wehner, Kölle, Kretzschmar and Prinz render it of great importance in the selection of water for domestic purposes to ascertain the amount of free carbonic acid present therein, and to test the behaviour of such water when in contact with the various metallic substances which it is likely to encounter. The above acid is readily soluble in water, but the degree of the solubility or the amount of the gas which water is capable of taking up depends somewhat on the temperature. The carbonic acid held in water only escapes from it very slowly when such water remains in a tranquil state, and the volume is but little altered by long keeping. The existence of the acid in most soils and in the atmosphere renders all surface water liable to contain carbonic acid, either free or in the combined state. It seems probable that the bulk of the carbonic acid gas contained in water is not derived from the atmosphere, but is the product of the decomposition of organic substances. If this acid encounters in the soil such bases as lime, magnesia, or iron, these substances combine with it, either wholly or partially, in accordance with the amount that may be present. The insoluble monocarbonates are converted by the acid into the hydrocarbonates or the bicarbonates, which are soluble, and if these bases are abundant the water is entirely freed from carbonic acid, and for this

reason the so-called hard waters do not, as a rule, contain free carbonic acid. In soft waters carbonic acid is often to be found in considerable quantities. Though the average amount of carbonic acid contained in water is 15 to 40 milligrams per litre, it may in some cases be present to the extent of over 150 milligrams per litre. From the hygienic point of view the existence in the water of this gas is desirable, as it no doubt tends to render it palatable for drinking purposes. Some account is given of the reactions which take place when water with carbonic acid in solution comes into contact with certain of the metals—lead, iron, copper, zinc and tin—which are used in connection with domestic supplies, and in the case of iron it is shown that this gas attacks the metal just as would a solution of a weak mineral acid. In the water-mains there is usually also a supply of atmospheric air which provides oxygen and tends to intensify the action of the carbonic acid on metals. With lead pipes the carbonic acid can only be injurious when oxygen also is present, and it is shown that lead is so poisonous that its use for pipes for drinking water should as far as possible be prohibited. Indications are given of the various methods of carrying out analyses to detect the presence of carbonic acid, both alone and in combination, in drinking water, and the Author points out how water may best be freed from this acid or how the acid may be neutralized by chemical and mechanical processes.

G. R. R.

Sterilization of Drinking-Water. Dr. K. Kutscher.

(Gesundheits-Ingenieur, Munich, 14 September, 1907, pp. 597–8.)

The results are given of a series of tests, carried out by the chemical section of the Royal Institute for Infectious Diseases, of an improved apparatus, introduced by Dr. Kade, for the sterilization of drinking-water. The apparatus comprises a receptacle for the water to be treated, which if it is very foul should undergo some preliminary process of straining through coarse canvas. The water passes thence to the boiler, consisting of a series of tubes which are heated by a petroleum burner. It is thereby sterilized, and on the principle of reversed current cooling is conducted thence through pipes where it is made to give up its heat to the incoming untreated water. By this means the temperature is rapidly reduced and the purified water is then passed through a filter of animal charcoal which deprives it of its flat, insipid taste, after which it is ready for use. Some tests were first made at various rates of delivery to verify the extent of the cooling, and at a yield of 17 to 22 gallons per hour, after being at work for ten minutes, the difference in temperature between the feed-water and the sterilized effluent was only 2° C., or at 35 gallons per hour 4° C. higher for the latter. The rate of working depends on enlarging or reducing the size of the petroleum flame. To test the germicide properties

of the apparatus, experiments were made with Berlin canal-water, containing in February 7,000 germs per cubic centimetre. Even at the lowest rate of yield there were still 30 living germs to the cubic centimetre, and so many as to render it impossible to count them at the higher rate of delivery. Tests were likewise made in the manner set forth with pathogenic bacteria and cholera germs, and it was found that at the higher rates of yield the cholera vibrios were still capable of growth on the gelatine plates. After undergoing some changes at the hands of the makers, the apparatus was further tested, but again it failed to sterilize completely the water when the yield was too rapid. It is suggested that it would be expedient to introduce some better regulation of yield than by the area of the flame. These results differ from those of Messrs. Giemsa and Mayer, who found that the apparatus could be relied on in all cases to yield a sterile effluent.

G. R. R.

Water-Purification at St. Louis, Mo. E. W. Wall.

(Proceedings, American Society of Civil Engineers, vol. xxxiii, pp. 758–83.)

The muddy water of the Mississippi and Ohio Rivers forms the supply to many townships located on their banks, but few of them adopt purification methods for rendering it free from the sediment which it contains. Before 1904 (the year of the Exhibition at St. Louis) the water was supplied to St. Louis direct from the river to the mains without purification, but in the autumn of that year clear water was to be obtained at the tap owing to successful purification methods which were instituted. The water-supply for the town is taken from the middle of the river, about 10 miles above the Eads bridge, and is conducted through a 7-foot tunnel to pumps which raise it into six settling-basins each 400 by 670 feet. As the water carries in suspension 20 to 6,000 parts per million, and 2,000 to 250,000 bacteria per cubic centimetre, it will be seen that effective purification would involve a plant of an extensive kind. In 1903 the attention of the Water Commissioner was called to the use of lime and sulphate of iron as coagulants in conjunction with mechanical filters elsewhere, and it was decided to try the effect of these coagulants on the water. After much experimenting the plans for a permanent coagulating-plant were prepared. This consists of a building containing eight circular bins of 20 feet inside diameter and 40 feet high for the storage of sulphate of iron and lime. These bins contain enough for 160 days' supply of sulphate, and 45 days' supply of lime. The chemicals are fed into daily-supply hoppers, each with a capacity of 900 cubic feet, and are carefully weighed out. The milk of lime flows from the mixing-tank into a heating-tank, and thence to a collecting-tank from which it is taken to the pumps. A complete system of conveyors is installed for the rapid handling of the materials. Two new settling-

basins of reinforced concrete are being constructed, the floors of which are composed of 9-inch concrete.

The addition of the sulphate of iron and lime to the water in sufficient quantities causes a precipitate which carries down the impurities with it. The amount of sulphate is 0·5 to 4 grammes per gallon of raw water and of lime 5 to 9 grammes. The population of St. Louis in 1900 was 575,000; in 1907 it was 700,000, and the effect of the purification has been to reduce materially the typhoid-fever rate. The average cost per 1,000,000 gallons of water treated (including labour and power) for the first year was 15*s.*, for the second year 16*s.* 9*d.*, and for the third year 19*s.* 4*d.* This increase in cost was largely due to the increased price of lime. The effect of the treatment is a softening of the water by a reduction of the carbonate of lime to 53 per cent. of that in the raw water, and of the magnesia to 57 per cent.

R. S. B.

Grottoes as Sources of Water-Pollution. E. A. Martel.

(La Nature, Paris, 19 October, 1907, pp. 327–30.)

It is pointed out that under the law of the 15th February, 1902, it is interdicted in France to throw the dead bodies of diseased animals into caverns or grottoes, but from the account given by the Author, with illustrations of his exploration of the Grotto des Corbeaux, in the forest of Bélesta, Ariège, this law is more honoured in the breach than in the observance. This grotto is situated in the midst of a forest, and though reputed to be unfathomable, it was found to be in reality much more accessible than many others of the same size. By means of a rope ladder 105 feet in length, the explorers lowered themselves down the shaft to the summit of a lengthy slope which inclined downwards at an angle of about 45° to a total depth of about 360 feet below the opening. The surface of this slope, which was covered with rocky debris, partly fallen from the roof of the cavern, was strewn with the decaying carcasses of animals in every stage of putrefaction, on which swarms of flies were feasting. There were evidences that at certain periods considerable volumes of storm-water flowed down this incline, and no doubt permeated the limestone rock at the base, which abounds with fissures and cracks. In this case the grotto is situated high up on the flanks of a limestone mountain, and some 1½ mile distant, at a lower level, is the famous intermittent spring of Fontestorbes, which is used as part of the water-supply of the commune of Bélesta. It is shown by hypothetical sections through the strata that the "broth of ptomaines" arising from the dead beasts, slaughtered because of cattle-plague, glanders, and other hideous diseases, is undoubtedly served up to the inhabitants of the surrounding villages for drinking purposes. Photographs are given to show the access to the underground cavern, the upper entrance

and the outflow of the Fontestorbes spring. It is stated by the Author that his illustrations show the whole process of contamination, and afford a strong proof of the urgent need of some enactment to render the cremation of infected cattle compulsory.

G. R. R.

Reinforced-Concrete Pipe for Carrying Water under Pressure.

C. W. SMITH.

(Proceedings, American Society of Civil Engineers, New York, vol. xxxiii, pp. 581–98.)

In connection with the Salt River project, Arizona, which is being carried out by the United States Reclamation Service, a canal 19 miles long, conveying 250 cubic feet of water per second, crossed two creeks, one the Pinto Creek, $\frac{1}{2}$ mile wide, and the other Cottonwood Canon, 250 feet wide. For these crossings two lines of reinforced-concrete pipe were employed, 5 feet 3 inches in diameter inside, making a total length of about 6,000 feet, the pipes being embedded in trenches beneath the beds of the creeks, which during a large part of the year are dry, though subject to heavy floods. The pipe for the Pinto Creek was made 6 inches thick and the Cottonwood pipe 7 inches thick. The reinforcement, longitudinal and transverse, consisted of $\frac{5}{8}$-inch steel rods, having an ultimate strength of 62,000 lbs. per square inch and an elastic limit of 30,000 lbs. per square inch. There were six longitudinal rods in the Pinto pipe and ten in the Cottonwood, and the transverse rods were spaced 3 inches apart. These were bent to the desired form in a bending-machine, the ends being lapped about 15 inches and bound with wire. The concrete was composed of 1 part Portland cement, $2\frac{1}{2}$ parts sand, and 4 parts fine gravel, and was mixed by hand, the initial setting taking place in $4\frac{1}{2}$ hours and the final in 12 hours. The tests required were as follows:—neat 7 days, 450 lbs.; 28 days, 550 lbs.; 3 months, 625 lbs.; 1 part cement, 3 parts sand, 7 days, 100 lbs.; 28 days, 200 lbs.; 3 months, 260 lbs.

A movable form was used in setting the pipe, constructed of steel plates in the form of a cylinder divided along a horizontal plane into two parts. The lower half could be dragged forward by horses on a light wooden track. The inner form for the upper half of the pipe consisted of 2-foot lengths of steel semi-circular cylinders, each in three pieces to facilitate handling. The outside lagging was made of $2\frac{1}{2}$-inch timber. Considerable trouble was experienced by the concrete peeling off with the upper stationary plates, and after many attempts to remedy this by coating the plates with various substances it was found that the best remedy was to allow the concrete to set more thoroughly than is usual. When the pipes were put into service leaks developed, in some cases in the form of longitudinal cracks $\frac{1}{16}$ inch to $\frac{3}{16}$ inch in width, which were invariably at the top of the pipe. This was attributed to the shrinking of the concrete in

setting, which was resisted by the steel reinforcing-rings, which were thus put into a condition of tension. Consequently on filling the pipe the concrete took the entire load until it failed, and then the steel took the load. The cost of the pipe per foot was 11*s*. Leakage-tests were made which showed the necessity for repairing the pipe at places. This was done by cutting out the cracks to a depth of 2 inches, which were then caulked with oakum and afterwards pointed with stiff mortar. The leakage under different heads of water is given in tables, and photographs show the trenches and method of constructing the pipes.

R. S. B.

Tapping Water-Mains under Pressure.

(American Machinist, New York, 1907, p. 679.)

This is a short article describing an apparatus by means of which large water-mains can be tapped, for making a connection, without turning off the water. The first step is to clamp around the water-main a special sleeve carrying a tee-branch to form the connection, and this sleeve is properly jointed in the usual way with yarn and lead. A gate-valve is then belted to the flange of the tee, and the outer flange of this valve is provided with slots for the bolts instead of holes. The tapping-machine is bolted to this flange and it consists of a mechanism for driving a boring-tool, or head, to make the necessary hole through the wall of the water-main, the gate-valve being the course opened for the purpose. The boring-bar of the tapping-machine is properly packed to prevent leakage, and is driven by hand in the smaller machines, and by means of a petrol-engine in the larger machines; this engine is bolted direct to the machine and drives the boring-bar by suitable reducing-gears. A photo-reproduction is given of one of the larger machines placed in a trench and making a 30-inch connection with a 30-inch water-main at Trenton, N.J. In this case a 1½-inch leading-drill was followed by a 4-inch cutter, to make room for the supporting-bar of the 30-inch cutter, of which a dimensioned drawing is given in the article. The large cutter was at work for 59 minutes, and in addition ½ hour was spent in making the smaller holes; the engine was rated at 6½ HP. and used 2½ gallons of petrol for the operation. The whole machine of this size, including the engine, weighs 5,500 lbs. The body of the large cutters is made of cast steel and is ⅛ inch less in diameter than the hole to be cut, and the cutting-teeth are spread outwards $\frac{1}{16}$ inch. Cutters below 8 inches are made out of solid tool-steel.

H. R. S.

Sewage-Purification Tests at Columbus, Ohio.

George W. Fuller.

(Journal of the Association of Engineering Societies, Philadelphia, August, 1907, vol. xxxix, pp. 67–130.)

Extensive sewage-purification experiments have been undertaken at Columbus, Ohio (population 175,000). Some of the problems arising out of these experiments are discussed in this Paper, and may be classified as follows:—(1) Preparatory Treatments, (2) Synopsis of Septic Treatment, (3) Intermittent Sand-Filters, (4) Contact-Filters, (5) Sprinkling-Filters, (6) Sedimentation and Filtration of Effluents of Coarse-Grain Filters, (7) Germicidal Treatments of the same.

For the preparatory treatment of the sewage, seven tanks, 40 feet long, 8 feet wide, and 8 feet average depth, were employed. Prior to treatment the sewage was screened through 0·5-inch and 0·375-inch mesh screens, and about 300 lbs. of wet screenings were removed for each million gallons of sewage pumped. This is equivalent to 0·2 cubic yard per million gallons (the corresponding figures for Paris are 0·25 cubic yard). These experiments showed that plain sedimentation will remove 60 to 65 per cent. of suspended matter and 30 to 35 per cent. of organic matter, and a velocity of flow of 0·16 inch per second is the maximum at which sedimentation can be properly carried on. None of the preparatory treatments will yield a non-putrescible effluent; for this the septic treatment is necessary, and it is in the septic tanks that the sewage is deprived of its objectionable qualities by the presence of bacteria. The method of treatment in the septic tanks differs in different places, and the Author cites the Birmingham and Plainfield, N.J., systems. In the former, suitably inoculated sludge is allowed to flow into the tanks before the introduction of the sewage, and in this way the process of decomposition is assured, but the intensity of septic action is dependent also on the temperature and consequently on the season. The septic treatment, in the Author's opinion, can be more satisfactorily applied to the highly-diluted sewage in America than to the strong sewage of Europe. For the same reason septic tanks in America may be without covers without causing a nuisance. The amount of sludge at Columbus was about 2·68 cubic yards per million gallons treated, and provision was made for depositing this material on land.

The contact-filters were 5 feet in depth, and, with a material ranging in size between 0·25 inch and 2 inches, they were capable of giving a non-putrescible effluent at a rate of 600,000 to 700,000 gallons per acre daily. These results are about the same as those obtained in Europe, notwithstanding the marked difference in the strength of the sewage. The most important point which the tests disclosed was the work of sprinkling-filters under temperatures of 10° below zero, which did not clog as might have been expected. The filtering material in the case ranged between 0·5 inch and 2 inches, but the Author recom-

mends 1 inch to 2·5 inches as the best size for maintaining a good circulation of air.

The Author recommends, as a test for putrescibility, that equal volumes of the effluent and tap-water be mixed and allowed to incubate at a fairly high temperature for 24 hours. A qualitative test is then made for dissolved oxygen, which if present is considered satisfactory. R. S. B.

Septic-Tank Process of Sewage-Purification. W. R. Butler.

(Transactions of the Canadian Society of Civil Engineers, Montreal, vol. xx, pp. 187–208.)

Satisfactory purification of sewage involves the thorough breaking up and oxidizing of organic compounds and putrescible matter, and their complete transformation without injurious consequences. As contact with the air alone is unable to effect this, minute organisms have to be added to assist, and all sewage-purification must therefore be subordinated to the requirements of micro-organisms. Crude sewage invariably contains the germs of living organisms which obtain their supply of oxygen from the process of breaking down the organic compounds. The experiments carried on by Mr. Cameron at Exeter, in which the sewage of St. Leonards was treated, form the basis of the septic-tank process of to-day, and the first city to adopt it was Exeter, with a population of 47,000. In this system the crude sewage was admitted directly into a large air-tight tank where decomposition took place by bacteriological agency. It was then allowed to flow out over a weir to a set of beds of filtering material where it came in contact with the air. The essential principle of this treatment is alternative periods of rest and aeration. Notwithstanding the large amount of solid matter in the crude sewage, the outflow is clear and colourless, and the solid matter is liquefied by the process. Experience shows that the efficiency of the system depends more on the mechanical properties of the material in the filter-beds than on its mineral characteristics, coke breeze or crushed slag being very good for the purpose. Experiments made by the addition of typhoid bacilli to the crude sewage showed that in the purified state less than 1 per cent. of them remained in the sewage after 14 days. In Canada the capital cost of the septic-tank process may be stated to lie between 14*s*. 6*d*. and 50*s*. per head for populations of 40,000 to 50,000. R. S. B.

Chamber-Settings at the Munich Gasworks. Riess.

(Journal für Gasbeleuchtung und Wasserversorgung, Munich, 3 August, 1907, pp. 717–21.)

These carbonizing-chambers are of much larger dimensions than the ordinary retort, each receiving a charge of 2½ to 3 tons of coal.

Each setting with its producer contains three chambers, which are set at an incline in order to allow of simple methods of charging and discharging. The producer is built below the charging-stage and in front of the setting. The system of heating is regenerative and efficient utilization of heat is attained, as shown by the fact that the average temperature in the setting between the sides of the chambers is 1,250° to 1,300° C., whilst the temperature of the waste gases leaving the setting is 350° C. The gas produced makes its exit at the top of the chamber and is conducted to the hydraulic main in the ordinary way; one hydraulic main serves for each setting and is thus fed by three pipes. The coke is run by gravitation direct to a pit formed outside the house, whence it is conveyed to store after being quenched. The operation of charging and discharging each chamber is carried out by four men and occupies about 3 minutes. The period of carbonization is 24 hours, and only one man is required in attendance during that time. For charging and discharging it will be seen that four men working 8 hours daily could easily manipulate ten to twelve settings of thirty to thirty-six chambers.

The installation at Munich consists of five settings each of three chambers, in which 495,000 to 530,000 cubic feet of gas are produced daily; this is equivalent to 99,000 to 106,000 cubic feet of gas per setting. These five settings are contained in one bench which is 67 feet long, 15·2 feet deep and 34·8 feet high. When carbonizing Saar coal a make of 11,870 cubic feet of gas per ton is realized, and the gross calorific value of the gas is 663 B.Th.U. per cubic foot. The quantity and quality of the by-products are superior to those obtained in retort-work. The yield of coke is about 67 per cent. and is in large pieces, somewhat harder and denser than retort-coke, although not so dense as to give any trouble to consumers. The production of breeze is very small, being only 1·8 per cent. The yield of tar and ammoniacal liquor is found to be about 13·4 per cent., of which 6·1 per cent. is tar. On an average 15·32 per cent. of the coke produced is employed for firing, but the Author hopes that this quantity will be eventually reduced.

No definite figures may be quoted as regards the yield of ammonia and cyanogen, but it appears that there is a substantial increase in the amount of ammonia produced, and a decrease in the quantity of cyanogen.

The main advantage to be gained in working with chamber-settings is to be found in the economy of labour. Three or four men are here required for 8 hours, whereas fifteen to thirty men are required day and night to deal with a similar make of gas in horizontal retorts.

E. V. E.

Position of Vertical Retorts in Regard to the Munich Carbonizing-Chambers. Dr. J. BUEB.

(Journal für Gasbeleuchtung und Wasserversorgung, Munich. 3 August, 1907, pp. 728-31.)

Some time back trials were undertaken for the German Continental Gas Company of horizontal carbonizing- and coking-chambers which had been erected for experimental purposes. Favourable results were obtained, but the work was not continued owing to more satisfactory results being obtained with vertical retorts. Mr. Riess has recently brought before the gas-industry a modified form of carbonizing-chamber [1] which is installed at the Munich Works. The main difference of this setting to those already used appears to be that it is set on the incline instead of horizontally, as is customary. This installation bears the same relation to ordinary coking-chambers as the Coze system of inclined retorts does to the ordinary horizontal retorts, and in the same way the only advantage seems to be that the chambers may be charged and discharged automatically. It has been usual to build chambers of 33 feet long, whereas the Munich chamber is only 13 to 16½ feet long, and it is evident that with an inclined chamber the length must necessarily be limited in order to reduce the cost of a high structure. In these chambers, as in others of similar type, a space exists above the coal which is very highly heated and through which the gas must travel on its exit. The amount of naphthalene produced is thus augmented to the detriment of the heavy hydrocarbons. Further, the heat of carbonization is relatively high (1,250°-1,300° C.) in order to obtain a high yield of gas, consequently there is a danger of decomposing the gas as it is generated.

The conditions of carbonization in vertical retorts are so arranged as to overcome these difficulties and a high yield of gas and ammonia is attained; moreover, there is an absence of naphthalene in both the gas and tar. At Dessau, experiments have been undertaken with an enlarged vertical retort. This retort somewhat resembles a coking-chamber, and the Author thinks that good results would be obtained by setting the Munich chambers quite vertical. It is claimed that the Munich chambers, having a carbonizing period of 24 hours, will require no night work. This can hardly be successful, for in the ordinary retort where relatively small quantities of coal are carbonized, the periods required to thoroughly carbonize different kinds of coal vary, and with these larger chambers the time required will vary more. Thus if Saar coal is carbonized in 24 hours, 30 hours will be necessary to work off Durham coal. Therefore, either the whole time system has to be regulated to suit the kind of coal carbonized, or the charges are to be worked off inefficiently. It is stated that a night attendant is necessary to act as watchman, but in this no direct advantage is to be gained over the vertical-retort system, as at Oberspree one retort-house hand is required each

[1] See preceding Abstract.

shift for charging and discharging the retorts. Again, charging every 24 hours will certainly lead to the production of a poorer gas during the night than during the day, which would probably necessitate the use of expensive mixing-apparatus.

E. V. E.

De Brouwer Charging- and Discharging-Machine.

Dr. J. Becker.

(Journal für Gasbeleuchtung und Wasserversorgung, Munich, 31 August, 1907, pp. 809–10.)

The main advantage to be gained by the introduction of De Brouwer machines in the retort-house is in the reduction of labour. At Kaiserlautern nine men were required to operate six settings with hand charging, whilst ten settings were charged by five men using machinery. At Lichtenburg, near Berlin, eighteen men were employed per 12 hours for an output of 353,000 cubic feet; by the introduction of machinery nine men are now required in 8-hour shifts for a similar output. Again, for a daily production of 495,000 cubic feet twenty-two men were required with hand labour; now this labour can be carried out by twelve men.

The conditions of carbonization are somewhat altered when retorts are mechanically charged, the coal having to be broken into smaller pieces. This has a tendency to increase the deposition of retort-carbon and the production of thick tar, together with a high yield of naphthalene, at the same time stoppage in ascension-pipes is augmented. By regulating the heat of carbonization these difficulties may be overcome, and it is found advantageous to work with slightly lower heats than is customary at the present day. As regards these difficulties, English coal gives considerable trouble on account of its natural smallness and dusty nature, and better results are obtainable by mixing German with English coal. Although contrary to what would be expected, the Author finds that when carbonizing all kinds of small coal, the best results are obtained by maintaining medium heats only, and allowing a short period of gasification. The general economy effected by the use of De Brouwer machines is shown by the fact that with hand-charging an output of 10,080 cubic feet per ton of coal carbonized was obtained, whilst with stoking-machinery this figure was increased to 10,436 cubic feet per ton.

The Author has found that two well-trained men required 80 seconds to charge 3½ cwt. of coal into an 11-foot 6-inch retort, whereas with a charging-machine and one attendant 8 to 10 seconds were found sufficient to effect the same amount of work. During the operation of discharging and charging by machinery each retort remains open two minutes less than when working by hand, which considerably reduces the loss of gas, and in part accounts for the increased make per ton of coal carbonized. Further, with De Brouwer charging-machines coal is thrown into the retort in a uniform

and even layer, which tends not only to a more uniform production of gas, but to obtaining a gas of higher quality.

The Author charged twenty-seven retorts, each with 3½ cwt. of coal, first by hand and then by machine. The period of distillation was 5½ hours and the temperature of carbonization about 1,180° C., and it was found that the yield of gas in the case of machine-charged retorts was considerably higher than with hand-charged retorts.

E. V. E.

Effect on the Quality of Coal-Gas of Long-Distance Transmission at High Pressure. H. Zollikofer.

(Journal für Gasbeleuchtung und Wasserversorgung, Munich, 31 August, 1907, pp. 812–14.)

The Author has carried out experiments to determine the effect produced on the quality of coal-gas by transmitting a distance of 6½ miles, under different pressures. The gas was delivered from the works at Goldach to the storage-station at St. Gallen through a 14-inch pipe by means of Encke blowers driven by electric motors. On the photometer, light was developed in flat-flame burners consuming approximately 5·3 cubic feet of gas per hour, and all readings were corrected to that figure. No corrections were made for barometer- and thermometer-readings, as the object of the investigation was to determine the absolute difference of the gas at Goldach and at St. Gallen; and the figures recorded in Table I are obtained under these conditions. However, from an average of barometer-readings taken at the two points it was found that a difference of 2·7 per cent. existed. This means that 100 volumes of gas measured at Goldach would occupy 102·9, or roughly 103 volumes at St. Gallen, at similar temperatures.

Table I.

Pressure of gas in inches .	5·5	5·6	10·1	15·2	20·8	30·9	44·5
Percentage difference of illuminating power at the two points of testing . .	−1·0	+5·9	−6·0	−6·0	−2·6	−2·8	−2·5

As regards the calorific value, the results were obtained by Junker calorimeters, and the readings in this case are stated both in terms of the absolute or uncorrected difference, and the relative difference, which latter is arrived at by correcting the results to standard conditions.

Table II.

Pressure of the gas in inches.	5·5	5·6	10·1	15·2	20·8	30·9	44·5
Relative percentage difference in calorific value .	+1·9	+3·1	+1·8	−0·5	+2·8	+4·0	+1·9
Absolute percentage difference in calorific value .	+4·1	+4·9	+3·0	+2·5	+4·2	+6·8	+4·8

In concluding, the Author states that although there appears to be a slight decrease in illuminating power and an increase in calorific power, the gas on the whole does not suffer any appreciable deterioration by its long-distance transmission at high pressure.

E. V. E.

Naphthalene-Removal from Coal-Gas by means of Oil-Washing.

F. PANNERTZ.

(Journal für Gasbeleuchtung und Wasserversorgung, Munich, 22 June, 1907, pp. 568–70.)

Experiments were undertaken to determine the effect of employing oils which contained more or less naphthalene, for the removal of that compound from coal-gas. An anthracene oil was used for the purpose, and one sample examined showed that on fractionating, 6·5 per cent. by volume distilled over between 0° and 200° C., and 5·2 per cent. between 200° and 270° C. and these distillates remained liquid on cooling; the density of the oil at 15° C. was 1·1132.

In the first experiment gas, freed from naphthalene and ammonia by means of picric acid and oxalic acid, was passed into washers containing anthracene oil in which known quantities of naphthalene had been dissolved. The gas was passed at a rate slightly above 1 cubic foot per hour and the experiment was carried out at the temperature of the room. The results obtained showed that when the clean gas was conducted through oil containing 5 per cent. of naphthalene, it afterwards contained 2·4 grains per 100 cubic feet, whereas when the oil contained 30 per cent. of naphthalene the gas carried 9·7 grains per 100 cubic feet. A like experiment carried out with air gave practically similar results.

The object of the second experiment was to determine the ability of an oil, free from naphthalene, to extract that compound from the gas. The gas, originally free from naphthalene, was passed through a washer containing oil in which known amounts of naphthalene had been dissolved, then through a clean oil, and finally tested for the quantity of naphthalene it contained. It was found that almost the whole of the naphthalene could be extracted by the clean oil.

In the third experiment gas was passed through two washers. In the oil of the first washer a larger quantity of naphthalene had been dissolved than in that of the second, these being the conditions often obtaining on the works. The following results were obtained:—

1st Washer. Naphthalene in Oil. Per Cent.	2nd Washer. Naphthalene in Oil. Per Cent.	Naphthalene in Gas. Grains per 100 Cubic Feet.
10	2	1·3
10	5	1·6
20	2	0·9
20	5	1·8
30	2	0·9
30	5	2·5
30	clean oil	0·04

It appears that the quantity of naphthalene contained in an oil proportionally influences the amount taken up by a gas free from naphthalene, an equilibrium being established. Also from these and other experiments, the Author advises that washing by anthracene oil should be undertaken in such a way that the oil in the final washer, or compartment of washer, does not contain much naphthalene, and, above all, he suggests that as much of the naphthalene as possible be extracted from the gas in the cooling and tar-extracting process, so that the oil-washers have no unnecessary work to do.

E. V. E.

Waste from Lowell Gas Company's Yard. A. T. SAFFORD.

(Journal of the Association of Engineering Societies, Philadelphia, vol. xxxix, pp. 169–198.)

This Paper describes the remedies undertaken by the Lowell Gas Company to prevent tarry waste matter and gas from causing a nuisance in the neighbourhood of the works, complaints having arisen in consequence of the sewers becoming charged, and the cellars of surrounding houses becoming filled with gas. There are three gasholders in the works with capacities in cubic feet of (1) 100,000, (2) 500,000, (3) 700,000, and also the remains of the foundation of another holder about the size of (3). The ground surrounding the holders had been frequently disturbed for the laying of pipes, and in places was composed of cinders and blind drains, and it was estimated that there were about 1,600,000 gallons of liquid of an objectionable nature in the soil. The city sewers are located close to the gasworks and at elevations which would allow liquid to flow from the works into them. The pits of the holders were originally full of water to seal the gas, but had gradually become filled with different liquids produced in the works. The capacity of these pits was about 6,000,000 gallons. In addition to these liquids there was about 35,400 gallons per day of other substances, such as ammoniacal liquor, tar, and oil, besides water-gas refuse, produced during the manufacture of the gas. The smells in the city sewers complained of were due to the escape of these substances through the ground.

The first step taken to remedy this condition was to pump all the liquid from the ground into which it had percolated from the various tanks and holders, and it was then discharged into the sewers after being purified by filtration through coke sedimentation-tanks of a combined capacity of 88,000 gallons. These were sufficient for handling about 9 days' supply, and were constructed of concrete, the total cost, including excavation, cutting out old walls, rock, building forms and concrete and pumping, being at the rate of 74s. per cubic yard. The concrete was only reinforced in a few places. Tight drains (6 inches to 18 inches in diameter) were laid and

designed for surface-water only. These drains carried the water from the yard and were designed on the assumption that 4 inches of rain might have to be removed in 24 hours, and that for part of the time it might fall at the rate of 2 inches an hour. The capacity of these drains is so much in excess of the discharge of wastes that they can readily carry them off. Finally the yard has been concreted —particularly round catch-basins—with a 3-inch base of stone and tar and a top-dressing of coal-tar and sand. The Author refers to a Paper by Mr. J. Radcliffe on similar troubles experienced from ammoniacal liquors, and the waste products from the gasworks at Sutton, Surrey.

R. S. B.

Production of High-Frequency Oscillations from the Electric Arc.

L. W. Austin.

(Bulletin of the Bureau of Standards, Washington, 1907, pp. 325–40.)

The Author, after giving a short summary of the work done by Duddell, Fessenden and others, describes some experiments he has made, both with low- and with high-potential arcs having inductance, capacity, and a shunt in the circuit, with the object of determining the frequency of the oscillations. For the first portion of the work a 240-volt, constant-potential circuit was used, and the frequency of the oscillations was measured by means of a resonance method by tuning a secondary circuit consisting of a variable air-condenser, an inductance and a hot-wire ammeter. Experiments with a graphite arc showed that the frequency increased with the current, and that there were three well-marked maxima corresponding to frequencies of 295,000, 580,000 and 910,000. It is pointed out that the ordinary formula $n = \frac{1}{2\pi\sqrt{CL}}$, based on the form of the oscillations being sinusoidal, gives, in the case under consideration, 260,000 oscillations. Experiments with an arc in hydrogen were made in the manner suggested by Poulsen by forming the arc in the interior of a gas-flame, and the important difference was noted that, whereas with the arc in air, only a very little amount of power could be extracted from the oscillatory circuit, considerable power could be obtained when the arc was in hydrogen; in one instance, sufficient to incandesce to full brightness a 110-volt 32-candle-power lamp. The arc was also tried in steam, the effects being similar to those obtained in hydrogen, and oscillations of great power were also observed by forming the arc under water. A series of tests were also made with a high-potential arc placed in compressed-air, using metallic electrodes. The current was 0·15 ampere and on open circuit the pressure was 4,500 volts. Frequencies exceeding 1,000,000 were easily obtained, but at 4,500,000 they became irregular. A con-

siderable amount of power could be obtained from the oscillatory circuit, in some cases as much as 60 per cent. of that calculated to be available. The effect of air-pressure on the oscillatory current is remarkable, and is shown diagrammatically when using silver electrodes. Above 5 atmospheres, with a frequency of 850,000, the oscillatory current was approximately 12 amperes ; at 4½ atmospheres the current began to diminish rapidly, and at 2 atmospheres it was only 2 amperes ; below this pressure the readings were too unsteady to take. The high-voltage arc is nearly free from the hissing noises of the low-voltage arc ; it is therefore well suited for wireless-telephone experiments.

H. R. S.

Heating of Copper Wires by Electric Currents.

A. E. Kennelly and E. R. Shepard.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 795–821.)

The research reported in the present Paper was undertaken to increase the scope and the precision of the measurements made by one of the Authors and reported on in 1889 and 1893. In the former investigation the temperature coefficient of resistivity of copper wires was taken as 0·388 per degree Centigrade; in the present investigation the figure of 0·42, adopted by the American Institute of Electrical Engineers, was substituted. The Stefan law of thermal radiation was employed in this investigation instead of that of Dulong and Petit. The wires tested were 2 to 6 metres long, and were placed in water, soil or wooden mouldings; the measurements on wires in air are reserved for a future occasion. Pressure-wires were soldered at each end of the wire under test, and its resistance was measured by comparison with a German-silver wire-grid divided into three permanently-connected groups which could be connected either in series or in parallel, as required. A differential galvanometer was used to obtain the balance. A theoretical investigation of the temperature-elevation of a conductor cooled entirely by thermal conduction is given, leading to the formula $\frac{\theta}{1 + a\,\theta} = \mathrm{K\,I}^2$, where K is a constant for a given diameter of conductor, if the thermal resistivity of the substance in which the wire is placed is constant (there is evidence in the present report to show that it is substantially so for a range of temperature of zero to 100° C.); a is the temperature coefficient of resistivity of the wire and is equal to $\frac{1}{238 + t}$, where t is the initial temperature of the wire (when t = 18° C., a = 0·0039).[1] θ is the

[1] This appears to be at variance with the previous statement.

difference of temperature between the internal and external surfaces of the insulating cylinder. If plotted logarithmically, that is, plotting log θ against log I, a straight line is obtained, and if, for practical purposes, the temperature elevation (θ) is taken to average about 50° C., the former reduces to $\theta = K\,I^{2\cdot 2}$, where 2·2 is the tangent of the angle the logarithmically-plotted straight line makes with the base. The observations were also plotted logarithmically, and in this way they were rapidly and accurately reduced, and a comparison with the above theoretical formula was obtained. In the case of rubber-covered and braided "code-wire" the exponent for I was 2·2, and therefore agreed well with the theory; in the case, however, of wires placed in wooden moulding the exponent was 2·29 and the agreement was not so good. In the case of wood moulding also, the heat generated has to pass through rubber, dry braiding, air-space, and wooden moulding, so that the combined thermal resistance is difficult to compute. As already stated the constant K depends on the diameter of the wire and on the insulating material in which it is imbedded. Thus for No. 16 braided rubber-covered wire placed in water it is 0·007, and for No. 14 it is 0·003, whereas for the same wires placed in wooden moulding it is 0·08 and 0·044 respectively. Several determinations of the thermal resistivity expressed in thermal-ohms-centimetres are given and the figure varies considerably; the lowest observed was 161·4 with quartz-sand mixed with 20 per cent. of water, and the highest was 2,373 with white dry cotton covering, which latter figure agrees with that given by Peclet for cotton-wool. The time required to attain the maximum temperature was also observed; as an average half the final temperature was reached in 150 seconds, and 90 per cent. of it in 1,500 to 2,000 seconds.

H. R. S.

Reduction of Earth-Currents from Electric-Railway Systems by means of Negative Feeders. GEORGE I. RHODES.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 149–65.)

The subject of the earth-currents produced by electric railways is of great importance, and the Author develops a theoretical method by means of which the relative efficiencies of different return feeder systems can be estimated. He first takes the simple case of a rail-return with the negative bus-bar grounded, and shows that the potential of the rails is represented by a parabola and that the leakage-current is the area of this parabola; the Author takes this as his standard of comparison. The equation he obtains is

$$V = \frac{\rho I}{s_i}\left(l - \frac{l^2}{2L}\right)$$

where V is the potential at any point referred to the bus-bar as zero.
ρ „ „ resistance per circular mil foot of copper.
I „ „ total current.
si „ „ equivalent conductivity of rails in circular mils of copper.
l „ „ distance from the power-station.
L „ „ total length of line.

Several other cases are then considered, as, for instance, the following: copper return of uniform section bonded to the rails at short intervals; a single insulated negative feeder connected to rails at the middle of the line and at the power-station; several insulated feeders with equal potentials at all feed-points. It appears that the leakage-current in all cases is decreased by about 80 per cent. in comparison with the standard, by insulating the negative bus-bar, and, by further suitably arranging the negative feeders, the leakage current can be reduced to 0·77 per cent. The Author gives in all cases the method of calculating the weight of copper required for the negative feeders.

H. R. S.

Motor-Generators v. Synchronous Converters. P. M. Lincoln.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 217–25.)

The relative advantages of motor-generators and synchronous converters are considered under the following heads:—Reliability, voltage-regulation, corrective effect, efficiency, cost, parallel operation and starting. As regards reliability the Author is of opinion that the advantage lies with the synchronous converter, because there is only one machine to get out of order instead of two, and the factor of safety of the insulation is greater. Synchronous converters are supposed to be the most liable to "bucking," but that is because they are more widely used under conditions which will cause bucking than are direct-current generators. As a personal opinion, the Author states that, in railway work, the number of hours out of service in a given time on account of defects inherent in the apparatus may be taken in the following proportions: 25-cycle synchronous converters, 10 hours; induction motor-generators, 14 hours; synchronous motor-generators, 17 hours. The Author limits the term "voltage-regulation" to the changes which may take place in the direct-current voltage. He discusses the causes which may produce these changes and the various methods adopted for regulation. As regards efficiency the following figures are submitted for a unit of 500 kilowatts capacity: 25-cycle synchronous converter, 93 per cent.; synchronous motor-generator, 85 per cent.; induction motor-generator, 84 per cent. From these efficiencies the Author calculates that it would be worth while to pay £14 per kilowatt more for converters than for motor-generators, assuming 18 hours

per day work and 25 per cent. average load. The synchronous converter has a great advantage in respect of cost; the Author puts it down as being only 60 per cent. that of the motor-generator. There is not much to choose as regards parallel operation and starting between the two kinds of converters. In the final summary the conclusion is come to that it is only in few cases that the motor-generator should be used in preference to the synchronous converter.

H. R. S.

Light from Gaseous Conductors within Glass Tubes: The Moore Light. D. McFarlan Moore.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 523–59.)

The Moore light consists in the practical application of the vacuum-tube. If the degree of vacuum is properly adjusted a most efficient light of low intensity (as compared with other illuminants) is obtained, but owing to the electric discharge the gas in the tube is gradually absorbed, and the degree of vacuum becomes higher; at first the light simply becomes unsteady, then there are violent spasmodic flickerings, and finally the light fails. The problem, therefore, was to devise an automatic means of replenishing the gas and maintain the vacuum in the tube to the right amount, namely 0·1 of a millimetre of mercury. Mr. Moore has solved the problem by what he calls a "feeder-valve." This valve consists of a carbon plug, which is sufficiently porous to allow gas to pass, but will not allow mercury to percolate. When, therefore, the plug is immersed in mercury the valve is closed, but the mercury can be withdrawn by the action of a solenoid placed in series with the transformer. When the pressure in the vacuum-tube is in the neighbourhood of 0·1 millimetre of mercury the current increases rapidly with a diminution of pressure. Hence, the solenoid can be set so as to withdraw the mercury by the increase in the current so soon as the vacuum is increased beyond the desired amount; the tip of the carbon plug is then exposed, and a small amount of gas is admitted into the vacuum-tube. In normal operation the carbon-tip is exposed and gas is admitted for about one second every minute. The colour of the light depends on the gas in the tube; atmospheric air gives a pinkish light; nitrogen, which can be obtained from the air by absorbing the oxygen by means of a little phosphorus, gives a yellow light, and carbonic acid gives a pure white light. The outfit for the Moore light consists of a terminal box, containing a step-up transformer to obtain the high voltage (about 3,000 to 12,000 volts according to the length of the tube) needed for the discharge in the vacuum-tube, as well as the "feeder-valve." The glass tube, 1¾ inch in diameter, starts from the transformer, is taken along the cornices of the room and returns to the transformer. At present the tubes in use are 40 to 220 feet in length; the former

length requires a 2-kilowatt transformer, and the latter a 4·5 kilowatt transformer. Several comparative illumination-tests between the Moore light and glow- and arc-lamps are given. As compared with the former the Moore light was 5·1 times more efficient, and as regards the latter with the yellow light (nitrogen-gas) the Moore light was 2·9 times, and with the white light (carbonic-acid gas) 1·5 times more efficient. Several curves are published, giving characteristics of the Moore light; amongst others it is shown that the efficiency remains constant for an indefinitely long period of time; the efficiency does not vary over a wide range of supply-voltage, and the light-intensity is directly proportional to the voltage. The temperature of the tube is about 100° F.; it is therefore a "cold" light. It is also of low intrinsic brilliancy compared with other lights, as will be seen from the following table:—

Moore light	at 6 hefners per foot . .	0·33
	„ 12 „ „ „ . .	0·66
	„ 36 „ „ „ . .	2·0
Cooper-Hewitt mercury tube		19·0
Incandescent filaments		250·0
Nernst glower		600·0
Arc crater		10,000·0

H. R. S.

Protection against Lightning, and the Multigap Lightning-Arrester.

DAVID B. RUSHMORE and D. DUBOIS.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 347–81.)

The term "Lightning" is used in the comprehensive sense, defined by Dr. Steinmetz, viz., "All phenomena of abnormal voltage and abnormal frequency." As a matter of general interest a number of photographs of lightning are produced. A number of the lightning-arresters in general use are briefly described. The horn lightning-arrester is not considered to be satisfactory, as although it protects the line it disturbs the conditions of operation, if it is used without resistance, for it then has the effect of a short circuit and the discharge will throw synchronous apparatus out of step. An example of the use of this arrester for the wood pole of a transmission-line is given. The Authors are of opinion that the water-jet arrester has obtained an undeserved reputation, and they do not recommend its use. An overhead grounded wire is strongly recommended, although the protection is not complete. Experience has shown that a grounded wire below the transmission-line is not so effective as one placed above; it should be of $\frac{3}{8}$-inch stranded steel-wire, grounded at least every 500 feet. In the case of choke-coils certain disturbances render a high reactance desirable, in others it is undesirable. It is therefore better to have a moderate reactance, sufficient to reduce the pressure at the station to within the

dielectric strength of the apparatus. The importance of a good "ground" is insisted upon, and if it cannot be obtained near the arrester and a better one could be obtained further off, the principal ground should be formed at the distant point and a secondary ground established immediately below the arrester. The remainder and principal portion of the article is devoted to a consideration of the multigap lightning-arrester, which consists of a number of metal cylinders placed between line and ground and between line and line; there is a small air-gap between each cylinder. It can discharge abnormal rises of potential without disturbing operating conditions; the "dynamic" current only following the discharge for one-half cycle. The theory of this arrester, described by means of diagrams, may be stated briefly as follows:—If there were no capacity between the cylinders and the ground the distribution of the potential applied across the arrester would follow a straight-line law, but the capacity between the cylinders and the ground distorts the distribution and makes the gradient of the fall more rapid for the cylinders remote from the grounded end of the arrester. The distortion is small for a few cylinders, but is considerable when the number is large. The effect is that at a certain voltage across the arrester the potential gradient between the first and second cylinder is sufficient to break down the dielectric, and an arc is produced; the gradient between the second and third cylinder is thus increased until it is sufficient to break down the dielectric, and an arc is produced; the gradient between the second and third cylinder is thus increased until it is sufficient to break down the dielectric, and this action will continue (if the initial disturbance is sufficient) until the arc has passed entirely across the arrester, when the "dynamic" current flows for half a cycle. The voltage which will maintain the arc in an alternating current depends on the temperature of the arc, which itself depends on the boiling-temperature of the metal forming the cathode; a low boiling-point is desirable, but the corresponding metals are liable to get out of shape under the action of the arc, and an alloy, with a less fusible metal, must therefore be used for the cylinders. Resistance is one of the important factors in lightning-arresters; if placed in series it limits the current but may be dangerous in case of a surge. High-frequency discharges will break down a number of gaps, but will not break down a number of gaps and a resistance combined. Low-frequency discharges are not much impeded by resistance, but can only break down a few gaps. To meet all cases a graded resistance lightning-arrester has been developed in which there are a number of gaps, a few of which are placed in parallel with a low resistance, a larger number with a medium resistance, a still larger number with a high resistance, and finally there are some cylinders without any resistance at all. These three resistances force discharges of different frequencies to select different paths, and thus give a breakdown-voltage of about the same value in each case. Designs for two such graded multigap arresters for a line voltage of 35,000 are given: one is suitable for delta and for ungrounded star transformer connection,

and the other for grounded star systems. They are also made for a variety of voltages varying between 6,600 and 60,000. A photo-reproduction of a discharge through one of the arresters is given, also some oscillograms.

H. R. S.

Practical Testing of Lightning-Arresters. PERCY H. THOMAS.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 915–55.)

The Standardization Committee of the American Institute of Electrical Engineers requested the preparation of this Paper, together with one by Mr. E. E. F. Creighton to serve as a basis for discussion. The Author is of opinion that it is safe to design lightning-arresters which will deal with static waves or surges of potential only a few times greater than that of the line in the case of extra high-tension circuits (say 40,000 volts on the line), and as to the frequency it will practically never reach 50,000,000 cycles per second. In the general considerations of the tests to be carried out it is pointed out that the determination of the voltage at which the arrester will discharge is of importance, first to find the minimum voltage at which protection begins, and secondly, to ascertain the margin of safety there is against continuous breakdown from the line-voltage itself. The voltage which multigap arresters will resist depends very largely on their surroundings, and if placed in the neighbourhood of other arresters, or of line wires, they may be quite unable to hold the normal voltage on the line. The Author is of opinion that laboratory tests are really only useful for research work, and should not be specified for testing commercial arresters, as it is manifestly impossible to reproduce the varying conditions to which the transmission-line is exposed. Several diagrams of the arrangement of the apparatus for testing lightning-arresters are given; for instance, that for determining the impedance of a lightning-arrester in which a static machine with Leyden jars is used for producing the discharge. In another arrangement a step-up transformer connected to an alternating-current main is employed. It is stated that satisfactory condensers for this work can be made with sheets of metal placed between oiled cambric or oiled paper which may be immersed in oil or not. The test for the non-arcking power of a lightning-arrester requires a full-size generator to supply the electrical energy, and a diagram of the connections is given. The Paper concludes with a list of six suggested tests.

H. R. S.

Electrical Transmission of Photographs. GUSTAVE WILL.

(Deutsche Techniker-Zeitung, Berlin, 1907, pp. 244–6 and 263–5.)

This article gives a complete description of a method elaborated by Professor Korn of Munich, by means of which half-toned pictures, such as a photograph, can be transmitted electrically. At the sending-end the strength of the electric current is regulated by the light passing through the picture and falling on to a selenium-resistance; at the receiving-end the depth of tone is determined by the strength of the current. The selenium-resistance consists of a thin sheet of insulating material, such as porcelain, round which two fine copper, silver or platinum wires have been wound to form electrodes. The sheet is raised to the melting-point of selenium, which is then thinly coated on so as to imbed the wires. After this the sheet is maintained for a long time at a temperature of 200° C. which crystallizes the selenium, and renders it conducting and sensitive to light. The picture to be transmitted is transparent and is mounted on a glass cylinder (or drum) fitted with a screw, by means of which it can be rotated and advanced one millimetre each rotation. A beam of light is focussed by means of a lens on to the picture, passes through and is reflected by a prism placed in the axis of the glass cylinder in the form of a cone of light embracing the selenium-resistance. The intensity of the light spread over the resistance depends on the depth of tone of the picture at the particular point, and hence the sending current (in the circuit of which the selenium-resistance is placed) will vary from point to point of the picture. This current passes through a galvanometer at the receiving-end, the deflections in the pointer of which cause variation in the resistance in an alternating current of 20,000 to 30,000 volts, thus varying the intensity of the light given by a small special lamp of the Geissler-tube type. This light falls on a sensitive film mounted on a drum which is rotated after the manner of a phonograph-drum. It is essential that the rotation of this drum and of the drum at the sending-station should synchronise accurately, and this is effected by regulating the speed of the electric motors, which drive the drums, by means of slip-rings connected to a frequency-indicator of the Hartmann-Kempf type. The motor at the receiving-end is regulated to run about 1 per cent. faster than the motor at the sending-end, but at each revolution the drum at the receiving-end is stopped, and is started again by means of a special current-wave. In this way the two drums are synchronized at every revolution. The special lamp, working at high voltage, was found not to give very satisfactory results, and subsequently an arrangement with a reflecting galvanometer was substituted; the depth of tone produced on the sensitive film was made to depend on the angle of deflection of the galvanometer. Some examples are given of photographs transmitted through 1,250 miles of line.

H. R. S.

Switchboard Practice for Voltages of 60,000 *and Upwards.*

STEPHEN Q. HAYES.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 989–1013.)

This Paper discusses in some detail the general considerations affecting power-house switchboards, but is limited to voltages of 60,000 and upwards when obtained by means of step-up transformers. The recent practice is to control the high-tension circuits by means of oil-switches, mounted on a switchboard, which may be of: (*a*) the panel type, (*b*) the pedestal type, or (*c*) the desk type. An example of the latter for controlling a 88,000-volt plant is given with a photo-reproduction. These oil-switches may be either bottom-connected or top-connected; the former has the disadvantage that the sediment tends to collect on the contacts, but it has the advantage that the operating-rod and the leads can be placed further apart. Examples, with dimensioned illustrations, of several designs of top-connected switches (or breakers) are given; one of these deals with 120,000 volts, and the oil-tanks are made of boiler-plates. The 60,000-volt switches for the Ontario Power Company are illustrated. They deal with 200,000 kilowatts, and are guaranteed to open safely under any conditions of over-load or short-circuit. Arguments for and against the enclosed and open system of bus-bars are given, and it is pointed out that, since the violence of an arc and the destructive effect of a short-circuit depend on the amount of current, these effects, for the same capacity of plant, would be much less for high voltage than for low, hence the advantage of enclosing is not so great. On the other hand, it is easier in the open system to obtain a greater striking distance, which is an advantage in the case of high voltages. Further, the enclosed system generally necessitates several floors or galleries, and a more expensive building is needed. Lastly, the inspection and repairs are more difficult to carry out in the enclosed system. Typical examples of both the enclosed and open systems are given with three dimensioned drawings. It is suggested that outdoor switching-arrangements may be adopted in the future.

H. R. S.

Developments in Wireless Telegraphy. DR. LEE DE FOREST.

(Journal of the Franklin Institute, Philadelphia, 1907, pp. 461–70.)

This Paper is partly an historical account of wireless telegraphy, beginning with the first actual application, about 9 years ago, of the Hertzian-wave telegraphy. Progress has been so rapid that the older methods are completely out of date and, directly the limitations of the coherer were recognized, six different kinds of receivers,

differing fundamentally in the physical and the chemical principles involved, have been discovered and commercially applied. Amongst these is the hot-wire "barreter," moderately sensitive, but "injuriously affected by a lightning storm in the next hemisphere." Another of these oscillation-detectors depends on the electro-thermic qualities of certain substances, thus the unequal heating effect produced at the junction of crystalline silicon and copper causes feeble local currents, capable of operating a telephone-receiver. In the sixth type of receiver described the wave-responsive element is a gas rendered conducting by heat or electricity, or both combined. Such receivers have been given the name of "Audion"; they are very sensitive and have extremely close tuning qualities. Attention is drawn to the use of horizontal antennæ in order to give direction to the waves and to experiments made with antennæ inclined 30°, with the same object in view, but the Author points out that the great problem of non-interference must be solved by syntonization, and that, if properly tuned, there will be no interference from foreign oscillations, differing only 1 per cent. in frequency, even if of fifty times greater intensity.

H. R. S.

Leipzig New Telephone-Exchange. WEBERSTEDT.

(Archiv für Post und Telegraphie, Berlin, 1907, pp. 257-89. 18 Figs.)

This is a very long and well-illustrated description of the new telephone department of the chief post-office at Leipzig. In October, 1898, the chief telephone-exchange was in the front of the post-office building, but now a special new building has been erected and the old apparatus has been entirely removed and an installation of the newest type put in by Messrs. Siemens and Halske, of Berlin.

The exchange itself is on the fifth floor, while the relay-room is on the floor below, and the dynamos and central batteries are fixed in adjoining buildings. The cables are carried underground in the central portion of the city and overhead in the outer part. Each cable-way is 3·94 inches in diameter and formed in a cement block; standard cables have two hundred and twenty-four or two hundred and fifty pairs of copper wires; each wire is 0·003 inch in diameter, and the cable is lead-covered and then iron-armoured, and about one hundred cables enter the exchange at present. The installation is calculated for 15,000 subscribers, but could easily be enlarged to accommodate 20,000 subscribers. At the present time 10,900 main instruments and 4,300 extensions are connected.

The instruments are illustrated and described in detail, and diagrams of connections are given. The current is supplied by an installation, comprising two 50-HP. Diesel engines, and the cooling water is used afterwards for spray-baths. Each engine drives a dynamo developing 110 to 168 volts for feeding the network, and also a second dynamo developing 27 to 58 volts for use in boosting

the voltage for battery-charging. The battery has a capacity of 540 ampere-hours, and is used while the engines are idle; besides this there are two batteries, each of sixty cells with a capacity of 435 ampere-hours on a 10-hour discharge. Each of the sixty-cell batteries is discharged with the cells grouped in ten sets of six cells each, but is charged with sixty cells in series. The cost of the work is not stated, nor are the subscription-rates quoted.

E. R. D.

Compensated Two-Circuit Electro-Dynamometer.

EDWARD B. ROSA.

(Bulletin of the Bureau of Standards, Washington, 1907, pp. 43–58.)

The Kelvin balance is a standard instrument for the measurement of alternating current, but when used for heavy currents of more than ordinary frequency, or having a distorted wave-shape, eddy-currents are produced which cause an appreciable error. An electrometer connected with a standardized shunt is subject to the same errors, but if the inductance of the shunt and the wave-form be known, a correction can be calculated. In the instrument described in this article the usual single coil is replaced by two coils arranged astatically, and it is then possible to compensate for inductance and capacity, and the instrument has the same constant for alternating as for direct current, and is further independent of frequency and wave-form. In the ideally simple instrument the self and mutual inductance of the moving-coil circuit, and the eddy-currents are supposed to be zero, and it can easily be shown that the ideal instrument is independent of the self-inductance of the shunt. In the actual instrument, however, the self-inductance of the potential circuit and the mutual inductance between the two circuits, as well as the eddy-currents, must be compensated for, and this can be done by inserting in the potential circuit a resistance in parallel with a condenser, so adjusted as to make the potential current have the same phase as the impressed electromotive force in the potential circuit. The method of determining this capacity and resistance is given in the article. The compensation for the eddy-currents can be obtained by inserting a few turns in series with the moving-coil circuit and placed in such a position that it will compensate for the mutual inductance due to the eddy-currents. An expression is obtained for the torque due to the eddy-currents, and the Author has experimentally verified this expression by placing coils of known resistance and inductance near the instrument. When all the compensations referred to above have been effected the instrument can be calibrated by means of a direct current. To vary the range of the instrument the resistance of the shunt can be altered above a certain minimum value, without disturbing the compensation, provided that the added resistance is free from capacity or inductance.

The shunt-resistance for a heavy-current instrument reading 100 to 1,000 amperes is submerged in oil cooled by a water-circulation. In such an instrument the field-coil consists of two turns of heavy stranded cable, and the deflections are read on a curved scale by means of a telescope. These two-circuit electro-dynamometers can be arranged as wattmeters.

H. R. S.

Discussion on Electrolysis.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 560–98.)

This discussion was based on three Papers by Messrs. Hayden, Knudson and Rhodes respectively.[1]

Mr. Frank N. Waterman criticized Mr. Knudson's experiments with concrete blocks, and so far from deducing from them that iron embedded in concrete is liable to corrosion from electrolysis, he thought they proved that the concrete protected the iron. He also pointed out that a current of 0·25 ampere per square foot which Mr. Knudson considered to be small, in reality amounted to 5,000 amperes per mile for a 30-inch water main, which was far in excess of actualities.

Mr. J. W. Corning gave the results of some experiments in which the return-drop to the power-station was observed, first when the distribution was on the two-wire single-trolley grounded-return system and then on the three-wire system. The results are given graphically and show, in general, a reduction of over 80 per cent. in the return-drop for the three-wire system.

Mr. S. M. Kintner reported tests agreeing with those of Mr. Hayden, and concluded that for iron and steel there is little or no electrolytic corrosion, and for lead it amounts to less than ½ per cent. of that caused by a direct current of the same value.

Mr. George S. Sever quoted the results of experiments, made at the Columbia University in 1904, which confirmed Mr. Hayden's tests.

Mr. Albert F. Ganz considered it impractical to adopt Mr. Hayden's suggestion to superimpose a small direct current to the alternating current so as to annul the chemical corrosion, but he agreed with the system of return feeders and negative boosters dealt with in Mr. Rhodes's Paper, and in confirmation quoted the German regulations on the subject.

Mr. C. P. Steinmetz said that lead-sheathed power-cables had little to fear from electrolysis, because the heat produced by the current in the conductor kept the cable warm and prevented condensation of moisture; telephone-cables, on the other hand, were subject to condensation, and he thought that even ½ per cent.

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[1] Minutes of Proceedings Inst. C.E., vol. clxx, pp. 459, 478.

of the corresponding direct-current electrolysis would be dangerous to such cables, and that they ought to be protected.

Mr. R. A. L. Snyder confirmed this opinion by quoting many cases in which telephone-cables had been corroded by alternating currents and had to be replaced.

H. R. S.

Polyphase Systems of Generation, Transmission and Distribution.

M. A. Sammett.

(Transactions of the Canadian Society of Civil Engineers, Montreal, vol. xx, pp. 237–56.)

The problem discussed in this Paper is whether two- or three-phase current should be employed for a transmission-line of 100 miles or less, with pressures up to and including 50,000 volts at the receiving-end, and also what the frequency should be. While the Paper is solely concerned with these questions, the scope is necessarily wide, for a railway system on the one hand, and a lighting system on the other, may require different treatment. The following are the principal points regarded:—(1) Reliability as to continuity of service; (2) Greatest efficiency as a whole; (3) Best attainable services for diverse classes of load.

Though two-phase generators are abandoned for hydro-electric stations, this system is somewhat simpler to operate for distribution-purposes than the three-phase, for the two phases may be controlled independently for single-phase lighting circuits. The three-phase generator is more efficient than the two-phase, while the only advantage the two-phase system has at the switchboard is the saving of one ammeter. All bus-bars, oil-switch contacts and switch compartments, cables from generators to switchboard and from board to transformers, are reduced in the ratio of 4 : 3, and while 15·6 per cent. larger cross-section of copper is required for the three-phase installation, the 25 per cent. saving in the number of parts will be in favour of the three-phase switchboards. The advantage of the three-phase system is further increased by the possibility of using transformer-connections by which the danger of resonance is eliminated. The distribution for this system is also simpler, and the Author does not regard the unbalanced condition which sometimes exists between the phases as a drawback.

The frequencies most widely used in America are 60 and 25 cycles. So far as the transmission-line is concerned, the lower the frequency the better the regulation, but the switchboard is unaffected thereby. Transformers, however, are more costly and are less efficient for 25 than for 60 cycles. For (1) incandescent lighting, 60 cycles is the standard, but lower frequencies are made use of for mercury-vapour lamps; for (2) induction-motors, 60 cycles has a decided advantage, giving a higher speed and consequently

lower cost; for (3) railway work the suitability of low-frequency synchronous converters is well established, as they are much more satisfactory than 60-cycle machines. In conclusion, under the conditions stated, for a mixed lighting- and power-load, with a railway-load not exceeding 32 per cent. of the total output, a three-phase 60-cycle system should be employed throughout.

R. S. B.

New Methods in High-Tension Line-Construction. H. W. Buck.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 981–7.)

Owing to the establishment of large generating-stations of the order of 100,000 HP. for transmitting energy to long distances, very high voltages have become necessary, and the present type of insulators has been outgrown. The Author describes a new method of line-construction which he has developed with Mr. E. M. Hewlett, who has devised a form of "linked" insulators, already described and illustrated.[1] At each steel tower the conductors are "dead-ended," and the line is connected across the insulator by a "jumper." The Hewlett insulator consists of a number of porcelain disks 10 inches in diameter and connected together by steel-wire links: each disk is suitable for a working voltage of 25,000 volts (when wet a disk will arc across at approximately 65,000 volts). For 100,000 volts four disks are linked together, and they can be placed either horizontally or vertically. A feature in the arrangement is that there is no torsional stress on the cross-arms. Descriptions are given, with diagrams, of two circuits which will soon be constructed, one to transmit 50,000 HP. 165 miles at 100,000 volts; the spans will be 500 to 1,000 feet. The other case is that of a two-circuit line for 80,000 volts, in which the spans will be 300 to 400 feet.

H. R. S.

High-Tension Outlets for Electrical Transmission-Lines.

Alvin Meyers.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 167–82.)

The outlet from a building for wires carrying a very high voltage-current has become of great importance, and difficulties have arisen in providing sufficient insulation, especially if the outlet is exposed to blowing rain or snow. It has been found impossible to provide, in this case, insulators of glass or porcelain having the necessary

[1] Proceedings American Institute of Electrical Engineers, 1907, pp. 775–9.

surface-resistance, and plate-glass with holes through the centre has failed owing to the leakage-current setting fire to the timber-construction. The Author describes and illustrates, by means of dimensioned figures and photographic reproductions, several designs of outlets which have been used with fairly good results. The first of these was employed by the Telluride Power Company, the voltage being 44,000. These outlets consisted essentially of a 4-inch by 4-inch oak timber about 5 feet long with a 1-inch hole bored through to carry the wire, which was incased in a hard rubber tube. The oak was carefully paraffined and the outlets were further protected by hoods fixed to the walls of the building. An experimental outlet is described in which the oak was replaced by a material called "fibre conduit." When dry this outlet withstood a voltage of 110,000, but when wetted to represent the effect of rain and snow, it failed at 23,000 volts, and a carbonized channel about 2 feet in length was formed. The next outlet described consisted of a fibre conduit filled in with ozokerite and having a porcelain insulator screwed on at the outer end to act as a hood. This form gave better results, but a weak point was found between the porcelain collar and the conduit which, whether wet or dry, broke down at 97,000 volts to 104,000 volts. The Author then gives the conditions which should be fulfilled by these high-tension outlets and supplies a complete specification for constructing and installing 44,000-volt outlet "bushings." These bushings were arranged to come out at the ridge of the roof instead of through one of the walls; otherwise their design is similar to those previously described. The specification gives minute details of the precautions to be taken in the manufacture, and of the insulation-tests to be carried out during manufacture. The first bushing made to the specification was placed in the sheet-iron roof of a transformer-house, and 100,000 volts were applied for three days, during which there were several severe wet snowstorms. After this 83,000 volts were applied for over three weeks, and during the whole of this time no appreciable change could be noted on a watt-meter recording the loss. The electrical properties of these new bushings are far superior to the previous ones, but the question as to the durability of the ozokerite requires further experience.

H. R. S.

Siegwart Reinforced-Concrete Posts for Electric Transmission-Lines. S. Herzog.

(Zeitschrift für Electrotechnik und Maschinenbau, Potsdam, 1907, pp. 147–51.)

These reinforced-concrete masts or posts, the invention of Mr. Hans Siegwart of Lucerne, are intended to carry electric power-transmission lines; they combine strength with lightness and cheapness, and are practically imperishable. Their cheapness results mainly from being manufactured by machinery in the following manner. The core consists of a longitudinally-split steel

tube of conical or other desired shape. The split is opened by a long wedge, and when the cement has set this wedge can be withdrawn so that the tube collapses to its natural dimensions and can easily be withdrawn. Longitudinal armouring, consisting of round steel rods, is placed around the tube and laced together by spirals of wire. The tube is then placed in a lathe driven by an electric motor and a cement band is wound round it. The machinery for doing this consists of a hopper to mix the cement and aggregate, and the concrete is delivered on to an endless band at a rate which can be regulated according to the thickness it is desired to make the mast. This endless band passes round the mast and applies the concrete under pressure over the armouring in the form of a spiral, regulated by the relative motions given to the rotating mast and to the concrete mixed and the endless band. Following the concrete band, a spiral of wire is wound on to the mast forming the outer armouring, and lastly a band of strong webbing is applied, which is removed as soon as the cement has set. These cement masts can be made of any length desired; and an account is given of some experiments made to test their strength and stiffness. The thick end was incased and a pull was applied to the other end through a dynamometer. The total length of the masts experimented on was 7·25 metres, and the free length was 6 metres. The external diameter at the bottom end was 430 millimetres, and at the top end 270 millimetres, the thickness of the cement walls was 30 millimetres, and the longitudinal armouring-rods were 7 millimetres in diameter. In one case failure took place when the force applied to the top of the column was 1,300 kilograms (23 cwt.), and the deflection was 310 millimetres.

H. R. S.

Transmission-Line Towers and Economical Spans.

D. R. Scholes.

(Proceedings of the American Institute of Electrical Engineers, New York, 1907, pp. 695–711.)

Transmission-line towers built up of steel angles, channels and tees are considered in this article, and a method of expressing the relation between the height, strength and cost of a tower is described. The first step is to design a tower suitable for given external loads and assumed wind-pressure, then, by a formula developed by the Author, the weight of a geometrically similar structure exposed to proportionate loads can be obtained. Thus, having designed a tower suitable for one span, that for any other span can readily be arrived at. The Author also investigates the most economical ratio between the base and the height, and finds that for spans of 500 feet it is 1 to 4. A numerical example is worked out suitable for a three-phase alternating-current circuit with conductors weighing 1·22 lbs. per foot. The temperature-range allowed for is 40° F. to 110° F.; it is supposed that ½-inch of ice may form around the

conductor, and the assumed wind-pressure is 30 lbs. per square foot. The various costs for 1,000 foot of line are worked out and, by means of curves, are shown separately in respect of the insulators, of the foundations, and of the tower structure itself, for spans of 100 to 1,000 feet. The combined cost is also shown by a curve from which it appears that the minimum cost is obtained with a span of 400 feet, and that there is not much variation for spans of 300 to 600 feet.

H. R. S.

Power-Production from Waste Gases. CH. DANTIN.

(Le Génie Civil, Paris, vol. li, pp. 137–42.)

The facility with which surplus power can be converted into electric energy, and in that form distributed over large areas with the minimum of loss, has given a great impetus to the employment of the waste gases both from blast-furnaces and from coking-ovens. For every ton of coal carbonized about 8,475 cubic feet of gas are set free, and in the more modern form of oven, only 65 per cent. of this is required for effecting the carbonization, the remaining 35 per cent. being available for outside purposes. The gas thus generated is of high calorific value, viz., 3,500 to 4,500 calories per cubic metre (393 to 506 B.Th.U. per cubic foot), its principal constituents being hydrogen and marsh-gas (CH_4); thus for every ton of coal carbonized 2,965 cubic feet of gas become available, which is equivalent to 3,707 cubic feet of a total value of 420,000 calories (1,666,560 B.Th.U.) per ton of coke produced. In the blast-furnaces about 63,560 cubic feet of gas are set free per ton of pig-iron produced, but here the value is as low as 950 calories per cubic metre (107 B.Th.U. per cubic foot): thus 1,710,000 calories (6,785,280 B.Th.U.) become available per ton of pig produced. A gas-engine of about 1,000-kilowatt power absorbs 3,600 calories per kilowatt; thus in a large works a very considerable power is available at a direct cost of little beyond the attendance on and the upkeep of the machines. The Cockerill Company of Seraing was the first to use waste gases on a large scale, and this Company is now distributing, from two stations, no less than 5,800 kilowatts after having supplied all the power and light required in connection with the works themselves. Assuming that the machines work 50 per cent. of the time, or 4,380 hours per annum, the total cost per kilowatt, including a sinking-fund so arranged as to extinguish the capital-outlay in 10 years and allow 5 per cent. interest on the loan, is 0·176*d*. This sum would of course increase or decrease according as fewer or more hours per annum were worked.

I. C. B.

Novel Hydro-Electric Power-House Construction.

(Electrical World, New York, 1907, vol. l, pp. 207–10.)

A new dam and power-house, possessing some novel features, have lately been constructed on the Patapsco River near Ilchester, U.S.A. The unique feature is that the power-house is inside the dam, which is not of solid masonry but of reinforced concrete. The dam is 220 feet long, 40 feet wide at the base, and 26½ feet high from normal tail-water to the crest. At each end the buttresses and deck of the dam rise to 10 feet above the spillway as a protection from floods and to afford convenient entrances to the interior of the dam. The spillway is 168 feet long and is fitted with anchor-bolts, whereby, if desirable, flash boards may be bolted on and the available head increased by 2 feet. The deck is supported by 19 buttresses, 24 inches thick at the bottom and 16 inches thick at the top, which are placed 12 feet apart. The shell of the dam is 18 inches thick at the bottom tapering to 10 inches at the top. The apron extends only half-way down from the crown, the remaining down-stream portion being entirely open and fitted with windows to admit light to the power-house. The apron is shaped so as to throw the water some distance away from the windows. The interior of the dam used as the power-house is fitted with a false ceiling hung 5 feet from the inside of the dam, so as to protect the apparatus from any water which might leak through. The water is fed to the turbines through steel pipes passing through the sheet of the spillway on the up-stream side 5½ feet below the crest, and is discharged by draft tubes into the base of the dam, dropping into a well sunk about 3 feet below the river-bed and passing out of the dam by a channel constructed in the river-bed. Two waste-gates are placed near the bottom of the dam, the water from which passes under the floor of the power-house.

The equipment of the power-house, with illustrations showing the illumination received through the windows under the fall is described in the Paper. This novel construction has the advantage of economy of cost and also of using the whole available fall. The power-house, though submerged, is quite dry and comfortable.

W. C. H.

Berlin Testing Institute for Heating- and Ventilating-Apparatus.

Dr. Karl Brabbée.

(Gesundheits-Ingenieur, Munich, 27 July, 1907, pp. 488–90.)

The available site of the new Institute in Berlin for the testing of apparatus for heating and ventilation at the Royal Technical High School is spoken of as being very confined for the objects contemplated by the founders, whose aims were briefly—to provide the means of

conducting scientific investigations, to exercise the students in the working of the apparatus and to render useful service to industry. Of chief importance was the attention to be given to experimental investigations, calculated to aid the progress and development of the science of heating and ventilation; such, for instance, as the estimation of the amount of heat given out by various heating-surfaces when the air-supply was brought into contact with them at high velocities, or when eddies and counter-currents were artificially created in the same. With respect to the work to be carried out by students, it was deemed advisable, in addition to the inculcation of theory in the class-room, to practise them in the actual working of the different kinds of apparatus, and in the methods adopted for testing them, as also for ascertaining their useful effect. In all cases it is intended that the students shall themselves conduct the experiments and record the results of their observations. Another matter considered as likely to be of importance, especially to those engaged in industry, would be the examination of the properties and merits of all kinds of apparatus employed for heating and ventilation, under actual working conditions, the results of these investigations to be properly attested and duly reported; such reports to be furnished on payment of prescribed fees, to cover the expenses incurred in the work. The buildings, which have been erected at a cost of £7,500, have a frontage of 367 feet, and comprise in the basement a large hall, 196 feet in length and 16·4 feet in height, which contains up to 1,412,000 cubic feet of air, capable of being subjected to any required pressure, say, to that of a column of 3·9 inches of water, and of being heated to a temperature not exceeding 30° C. A special electric motor is installed to drive the fans and the various arrangements for the testing of air-pressures, air-velocity, temperature, volume, etc., are very complete. Boilers are provided in which steam can be generated at a great range of pressures, and there is a fan available for testing purposes which can be driven so as to extract up to 423,000 cubic feet per hour. Numerous laboratories for special investigations have been fitted up, and all kinds of apparatus used for the purposes of heating and ventilation, as also for cooling the air, can be examined in regular operation. A number of the leading manufacturers have already placed specimens of their apparatus at the disposal of the authorities, and it is claimed that the Institute, though small, will be capable of yielding useful and valuable results.

G. R. R.

Electrolytic Manufacture of Alkaline Chlorates. G. Rosset.

(L'Éclairage Électrique, Paris, 1907, vol. lii, pp. 109, 181 and 224.)

This Paper opens with an historical review of the growth of the industry for the manufacture of chlorate of potash since that salt was first obtained, showing the purposes for which it was employed

—notably, in the making of matches—and describing briefly the methods of manufacture before the electro-chemical process was adopted. The Author next discusses some properties of the alkaline chlorates which are of importance from the point of view of manufacture by electro-chemical methods, and then enters on a detailed explanation of the actual manufacture by the Gall and Montlaur process, including information on the cost of an actual installation using water-power.

W. C. H.

Col d'Olen Observatory. E. A. Martel.

(La Nature, Paris, 17 August, 1907, pp. 177–8.)

The new buildings, constituting the Institute of the Col d'Olen on Monte Rosa, to be inaugurated officially on the 27th August, have been erected by means of contributions from the Queen Mother, the King of Italy, and a long list of subscribers, for the study of botany, bacteriology, zoology, physiology, terrestrial physics and meteorology. The Governments of France, Germany, Austria-Hungary and the United States have also subscribed to the undertaking, and each Government contributing £200 to the funds has the right to the use of one of the sleeping apartments and a table in the various laboratories. The Col d'Olen is on the south slope of Monte Rosa, between the Valleys of Gressoney and d'Alagna. The enterprise has been mainly promoted by Professor Angelo Mosso, of Turin, who is the President of the Commission; and the buildings shown in a photograph are situated at a height of 9,842 feet above sea-level. The Institute consists of three stories, and the central laboratory on the ground floor is set apart for physiology and bacteriology. At the back are the laboratories for botany and zoology, and on the same floor are the dining-room and kitchen, also a workshop and a store for instruments and apparatus. On the first floor is the library and fifteen bedrooms, and on the second floor are the rooms set apart for meteorology and terrestrial physics. The servants and staff are provided for in a wooden building adjoining the Institute. Every person inscribed as a resident has a right to a bedroom free of charge, a place in the laboratories and free use of the resources of the establishment, the library and the dining hall. To cover the charge of working, lighting, use of gas in the laboratories, etc., a daily sum of 2 francs must be paid by each resident. The laboratories of Monte Rosa, which have been erected at a cost of £4,700, will thus constitute a very perfectly-organized international station for scientific research, and they are undoubtedly destined to render important services.

G. R. R.

New Vacuum Disinfecting-Apparatus. Prof. Dr. Martin Hahn.

(Gesundheits-Ingenieur, Munich, 7 September, 1907, pp. 581-5.)

Special reference is made to the frequent complaints caused by injuries to materials and clothing in the process of disinfection in public establishments, and to the serious waste of blankets and woollen garments in the case of hospitals and infirmaries where disinfection has to be carried out very frequently. It is stated that many attempts have been made to overcome these drawbacks. The recent investigations of Esmarch, Kokubo, Mayer, Herzog and others are discussed, and it is asserted that by creating a partial vacuum at temperatures of 65° C. to 80° C., and subsequently introducing volatile disinfectants, more especially formic aldehyde, greatly improved results can be obtained. The fact, ascertained by Rubner, that, at comparatively low temperatures, if steam is blown into the disinfecting fluid it becomes possible to produce sufficient gaseous matters to effect satisfactory disinfection without too greatly reducing the penetration-power of the steam, is also quoted. By reference to diagrams the arrangements of a disinfecting-apparatus made by Schmidt-Weimar Brothers, in accordance with the researches extending over many years carried on by Dr. Pfeiffer, is explained. The results of numerous tests of this apparatus are given in a tabular form. The test objects were rugs, blankets and bedding in very thick bundles, and to prove the thoroughness of the process of disinfection spores of anthrax, done up in screws of blotting paper, were inserted into the midst of the bundles. These spores had been shown to be capable of withstanding for 4½ to 5 minutes an exposure to a jet of steam at 100° C. The mode of carrying out the tests is explained. As the outcome of the various experiments the Author recommends that for very densely packed bundles there should be an exposure of 1 hour to a temperature of 73° C., or for garments and lightly-folded materials an exposure of half-an-hour will suffice. The apparatus was found to be capable of effecting perfect disinfection if properly worked, and it can also be used as an ordinary disinfecting-apparatus with steam at 100° C. For very thorough disinfection, it is stated that a temperature of 74° with internal vacuum and exposure to steam and formalin gives perfectly reliable results.

G. R. R.

Disinfecting-Paints, with Special Reference to Vitralin.

Dr. E. Huhs.

(Zeitschrift für Hygiene, Leipzig, vol. lvi, pt. 3, pp. 329-42.)

Reference is made to numerous investigations conducted during recent years into the merits of various kinds of disinfecting- and germ-destroying paints. It is pointed out that certain porcelain

enamel paints, introduced under the names of Pefton and Vitralpef, have shown themselves to be possessed of considerable disinfecting value, and other compositions which are mentioned have been found to be superior to the distemper and similar washes commonly used. The method of carrying out the former trials is explained, and the effect of the paints on the various test-bacilli is discussed. The causes of the bactericide properties of these preparations are also examined, and it is stated that their advantages are partly due to the hard smooth surface they produce, as, in the case of rough porous wall-coverings, not only do the bacteria obtain a good hold, but dust and dirt are deposited along with them and afford the means for their propagation. Samples of enamel colours were furnished by the makers, Messrs. Rosenzweig and Baumann, of Kassel, for these trials, and the walls of the laboratory were, as to one half, painted with Vitralpef B, and the other half with Vitralin, three coats of each being applied at intervals of three days. The micro-organisms selected for the tests were, (1) bacillus prodigiosus, (2) staphylococcus pyogenes aureus, and (3) tuberculous sputum. The Author explains the method in which the cultivations were prepared, absorbed into pellets of sterilized cotton-wool and stroked over the wall, so as to produce a thin film of the desired test-substance. The sputum was painted on with a brush. After a definite interval the walls were well rubbed with a linen cloth steeped in sterile bouillon, and the resulting fluid was injected into guinea-pigs. The first tests were taken directly after the wall-surface was dried, the second set 7 weeks later, the third set in 3 months, and the fourth set was taken after the expiration of 12 months. The results obtained are shown in numerous Tables, and are compared with tests on common oil-paint and distemper. As a general conclusion on the outcome of all the tests, the Author states that Vitralin is superior to Pefton and Vitralpef, and that these colours all possess in a marked degree the disinfecting properties claimed for them.

G. R. R.

Establishment of the Thermodynamic Scale of Temperature by means of the Constant-Pressure Thermometer.

EDGAR BUCKINGHAM.

(Bulletin of the Bureau of Standards, Washington, 1907, pp. 237–93.)

There are two principal methods of gas-thermometry in use, namely, the constant-pressure and the constant-volume. The international "normal scale" is that of the constant-volume hydrogen-thermometer maintained at the International Bureau of Weights and Measures. Hydrogen gas, however, cannot be used even at moderately high temperatures; on the other hand, nitrogen is

suitable for high temperatures, but is unsuitable for low. There appears to be no gas known which is satisfactory for the entire range of thermometry, and it would, therefore, be preferable to refer all temperatures to a more general scale, such as the thermodynamic scale proposed long since by Lord Kelvin. If T′, T and θ are the numerical values on the constant-volume, the constant-pressure, and the thermodynamic scale respectively, then with ideal gas $\frac{T'}{T'_0} = \frac{T}{T_0} = \frac{\theta}{\theta_0}$. The ideal gas is defined as one which follows Boyle's Law and in which free expansion causes no change of temperature. These conditions are approximately satisfied by the gases used in thermometry, hence the three scales will be approximately the same, and the difference between them could be determined if the departure of the actual gases from the ideal conditions were sufficiently accurately known, which unfortunately is not the case. If both these departures from ideality could be determined separately it would be possible to establish a theory of the constant-volume thermometer. The Joule-Thomson effect (porous-plug) depends on the combination of both departures, and by its means it is possible to obtain a theory of the constant-pressure thermometer. This the Author proceeds to do, and concludes that the difference in the numerical values of any temperature expressed on the constant-pressure scale and on the thermodynamic scale is very nearly proportional to the constant pressure of the gas in the thermometer. Hence, if the corrections for any pressure have been obtained, they can be applied for any other pressure for the same gas. After considering the latest data available in connection with the specific volume, specific heat and coefficient of expansion of air, nitrogen and hydrogen, a lengthy investigation is given, in order to co-ordinate the experimental data available for the Joule-Thomson effect. It is shown that, when these data are reduced separately for each gas, much is left to be desired, but by applying the law of "corresponding states," which was first announced by Van der Waal, the observations for all the gases may be co-ordinated and represented by a single empirical formula, which, when plotted, gives a smooth curve along which the observations lie with considerable accuracy. With the help of this formula the integration of the equation of the constant-pressure thermometer can be effected, and therefrom the corrections to be applied to a constant-pressure gas-thermometer to reduce it to the thermodynamic scale can be obtained. The Author further obtains the relation of the constant-volume to the constant-pressure thermometer, and is therefore able also to give the corrections needed to reduce the readings from a constant-volume gas-thermometer to the thermodynamic scale. The results are exhibited graphically for the nitrogen scale, and are compared with those obtained by Berthelot and Callendar. The Author has carried his results up to 1,200° C., and at this temperature the correction for the constant-volume thermometer is + 0·93° C., and for the constant-pressure thermometer + 2·3° C. During the investigation

the Author determines the thermodynamic absolute temperature of the ice point, and states that its value is probably not far from 273·13°. As a check on his investigation he also calculates the thermodynamic centigrade temperature of the boiling-point of sulphur from the experiments of Callendar and Griffiths, obtaining 444·90°, whereas from the experiments of Chappuis and Harker he gets the figure 444·86°.

H. R. S.

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TO THE

MINUTES OF PROCEEDINGS,

1907–1908.—Part I.

N.B.—Titles in italics refer to Original Papers, and those selected for printing only are further distinguished by the suffix "(S.)" or "(St.)," the latter denoting a Student's Paper. Abstracted Papers are not so indicated.

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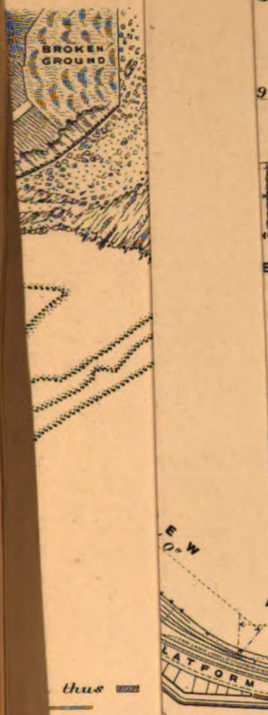

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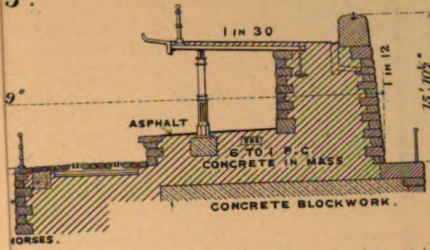

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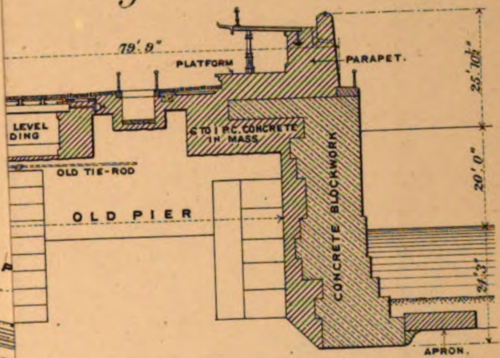

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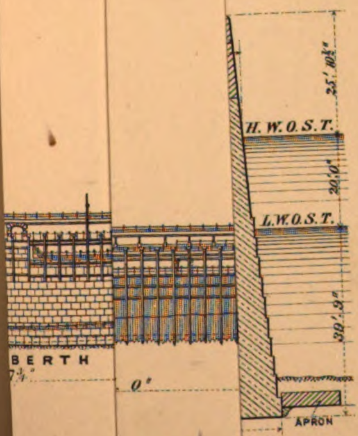

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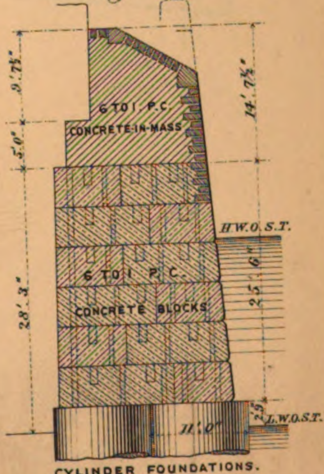

STRENGTHENING OF WEST SIDE OF PIER AT ROOT.

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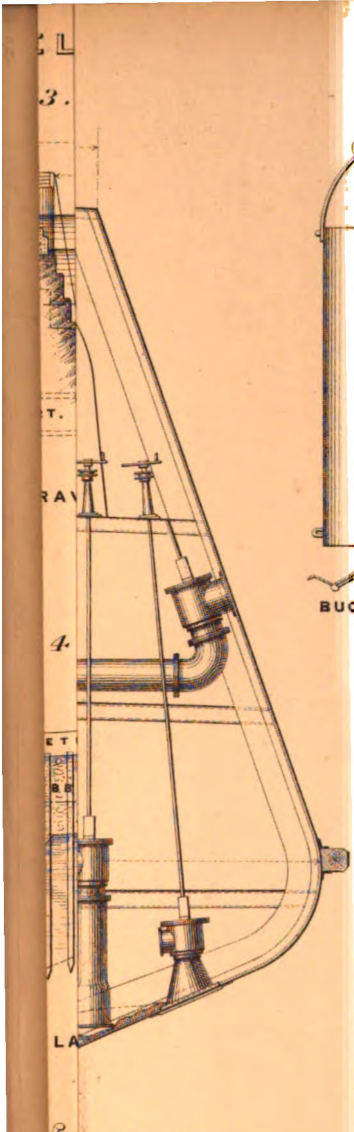
THOS KELL & SON, LITH. 40 KING ST COVENT



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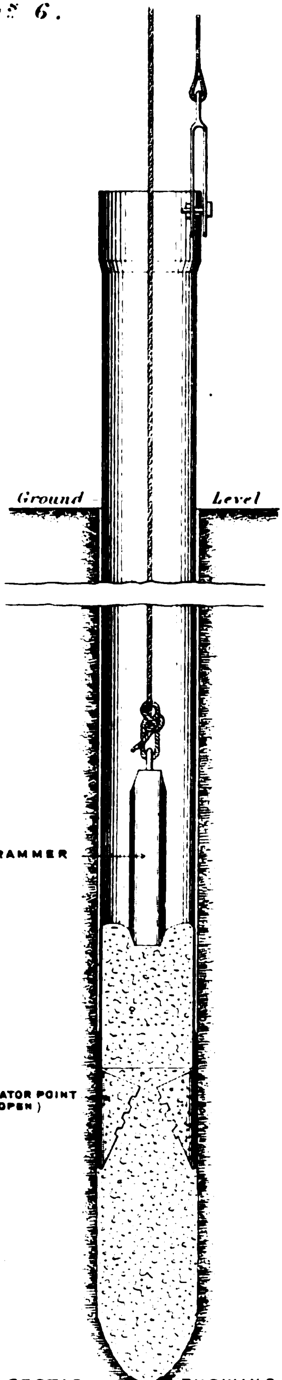

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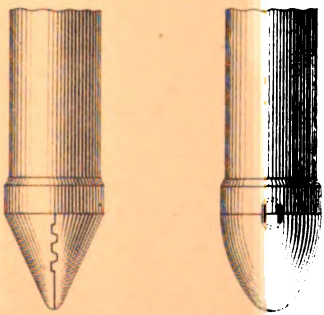

ALLIGATOR POINT (CLOSED)

THOS KELL & SON LITH

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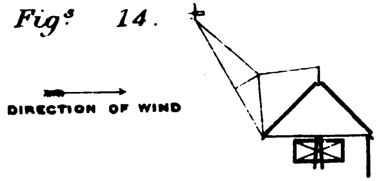

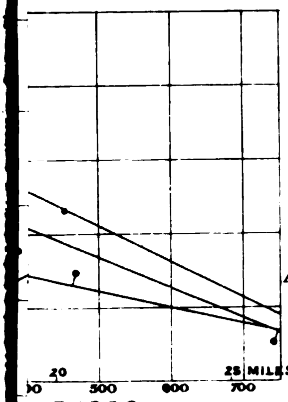

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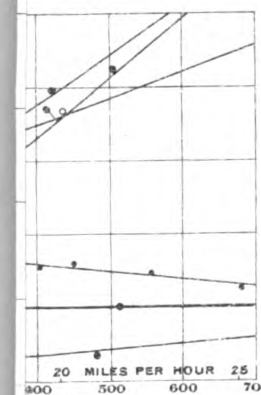

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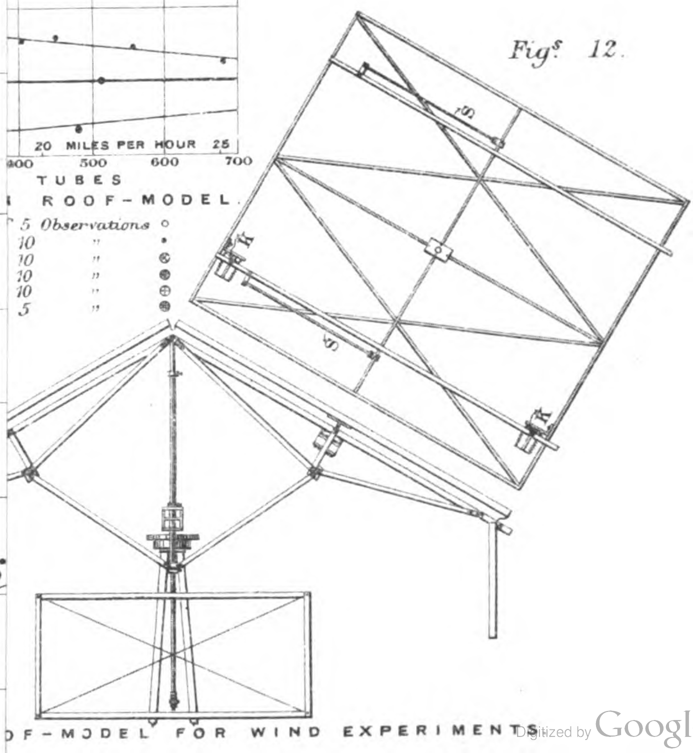

OF-MODEL FOR WIND EXPERIMENTS

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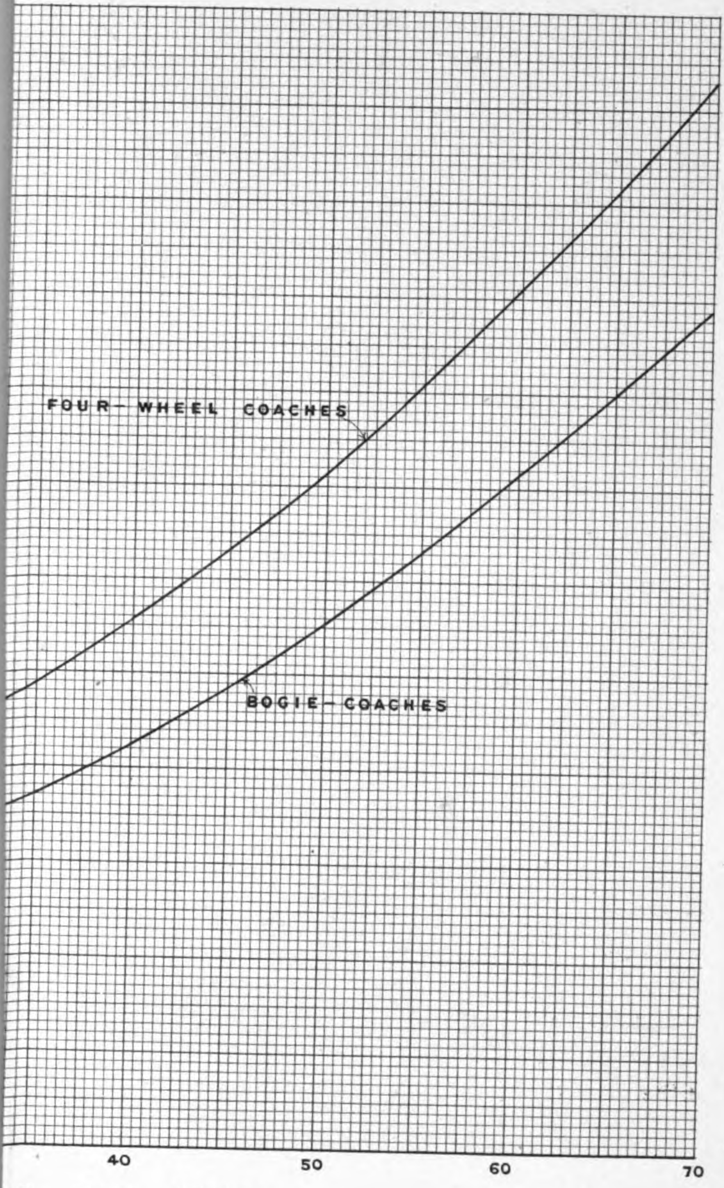

THOS KELL & SON, LITH. LONDON.

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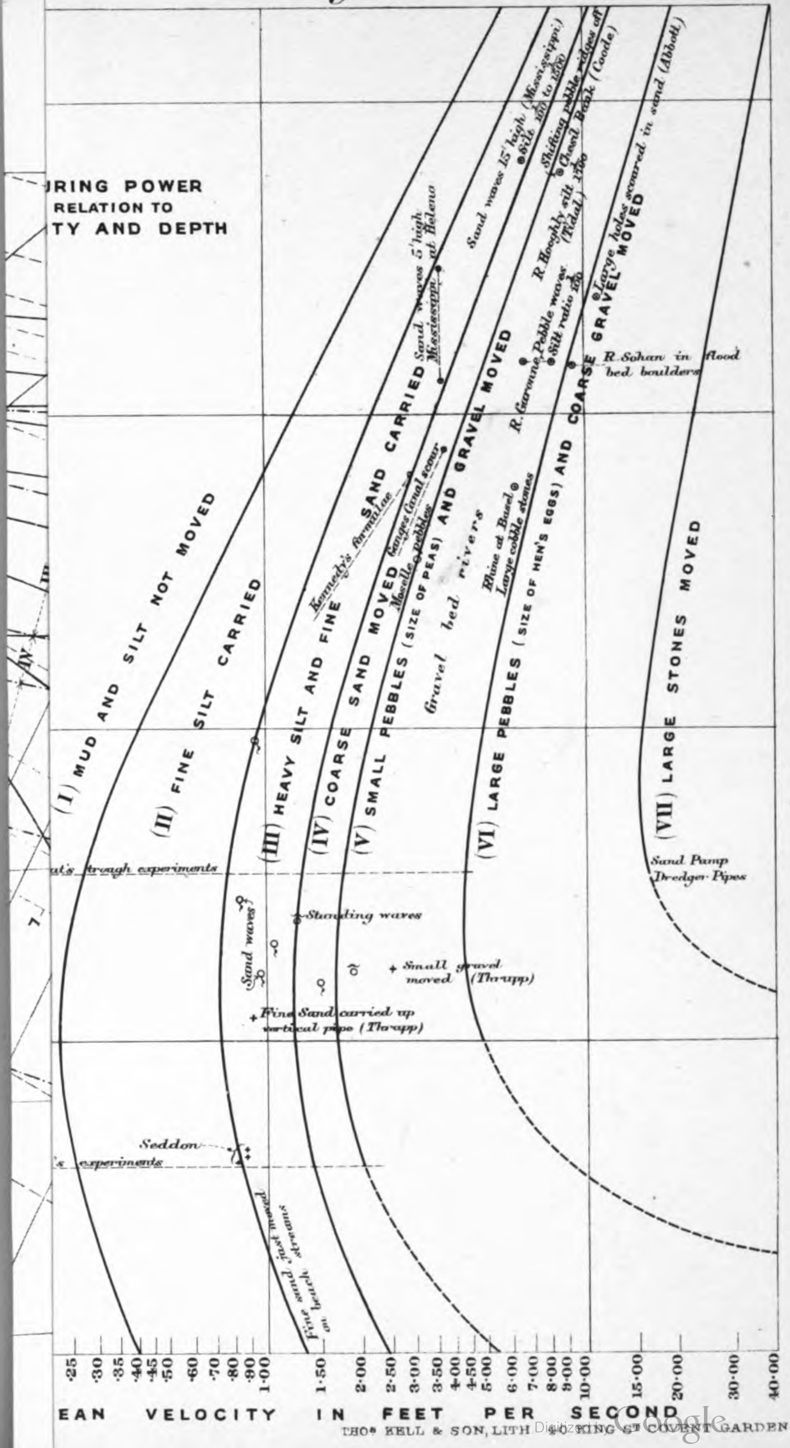

THOS KELL & SON, LITH 40 KING ST COVENT GARDEN.

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